THE

GEOLOGICAL MAGAZINE.

NEW SERIES. DECADE IV. VOL. III.

No. X.—OCTOBER, 1896.

ORIGINAL ARTICLES.

I.—Preliminary Notice on Fossil Monkeys from Madagascar.

By Dr. C. I. Forsyth Major, C.M.Z.S.

During my excavations in the marshes of Sirabé (Vakinankaratra District, Central Madagascar), my assistant, Mr. A. Robert, came upon a small fragment of a right upper jaw of a mammal, containing the two anterior true molars, which presented the well-known pattern of the Old-World Cercopithecidæ. The presence of true monkeys, as contemporaries of the *Æpyornis*, in very recent deposits, appeared to be such an extraordinary fact that I anxiously looked forward for further and more conclusive evidence. This fortunately has since been forthcoming, by the subsequent discovery of more complete remains. Two of the most important pieces, viz. the anterior part of a skull, broken off behind the nasals and the molar series, and a left mandibular ramus, are here figured and briefly described.

Fig. 1.—Profile of the anterior part of a skull of *Nesopithecus Roberti*.

In the fragment of the skull the orbits are directed straight forward, and, although their hinder portion is broken, clearly show that they were separated from the temporal fossæ by a bony wall. The lachrymal foramen is situated within the margin of the orbit. The inner upper incisors are in contact in the middle line. The nasals are concave in profile, and the facial profile is steep.

The above characters, together with the pattern of the molars, are the very opposite of what we find in Lemuroidea, whilst they are as positively evidence that the fossil belongs to the Anthropoidea. The nasals are broad, and so is the whole of the interorbital region; its transversal diameter almost equalling that of the orbits, and therefore exceeding what obtains in the genera of Anthropoidea, which show the maximum of lateral extension of this region (*Mycetes, Hylobates, Homo*). This is about the only point in which the fossil approaches some of the Lemuroidea.

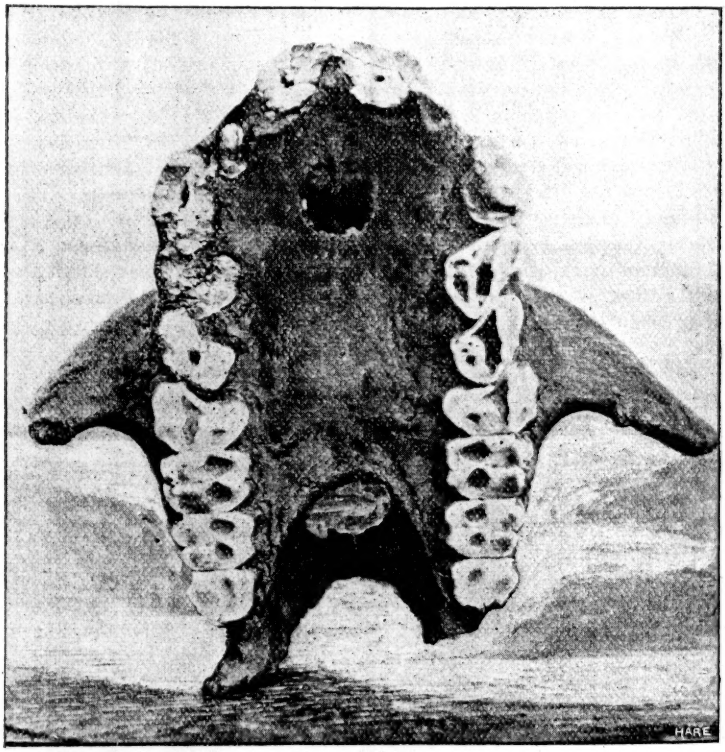


Fig. 2.—Palatal view of same skull of *Nesopithecus Roberti*.

The dental formula of the upper jaw is as follows: I 2, C 1, P 3, M 3. Only the molars, premolars, canine and the outer incisor of the left side are preserved, the other teeth being broken down to their sockets; those preserved are very much worn. The internal pair of upper

incisors were strongly developed, the outer being much smaller. The incisors being inserted obliquely, there is left a triangular gap between them and the strong canine. This last is provided with an internal basal cingulum, terminating in a small posterior cusp. The two anterior premolars have their longest diameter parallel to the long axis of the skull, whilst the third is longest in a transverse direction. They overlap each other, the first two being inserted obliquely in their alveoli. All of them are provided with an internal cingulum. The three molars are each composed of four tubercles, the outer and inner pairs being placed opposite one another and connected together by transverse ridges. This is the pattern of the Cercopithecidæ; but, unlike the Old-World monkeys, the molars decrease in size from before backwards.

The left mandibular ramus of another specimen, with less worn teeth, seems referable to the same species. The incisors

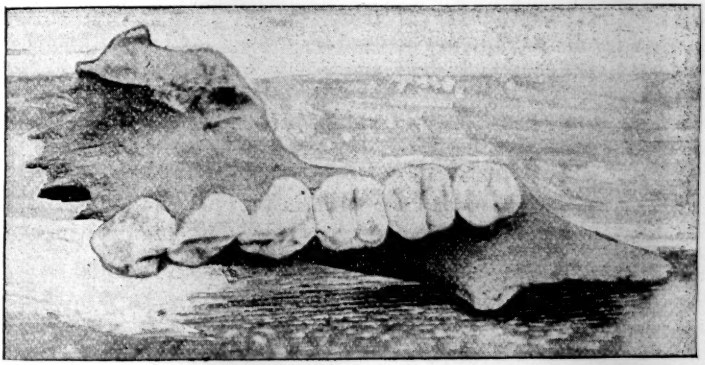


Fig. 3.—Left mandibular ramus of another specimen of *Nesopithecus Roberti*.

are wanting; but from the alveoli of both sides preserved, it is seen that they were four in all, and inserted obliquely. The remainder of the teeth of the left side are six in number: three are molars, their patterns being after the type of those in the upper jaw, and likewise decreasing in size from before backwards; no *talon* to the last inferior molar. Situated between the incisors and the molars are three teeth, viz. one less than in the upper jaw; and the question arises, whether the missing tooth is the canine or one of the premolars. The anterior of the three teeth in question is caniniform; but, in adapting the lower dental series to the upper one, it may be clearly seen that this caniniform tooth occupied the place behind the upper canine; so that, according to our received definitions, this tooth is not to be considered a canine, but a premolar. At any rate, whilst the dental formula of the upper teeth agrees with that of the Cebidæ, it is quite peculiar in the lower jaw; and whilst the pattern of the molars is that of the Cercopithecidæ, the premolars differ alike from Old and New World monkeys. The two posterior lower

premolars overlap each other, as in the upper jaw, their longest diameter being oblique to the long axis of the skull; the anterior of the two has a longitudinal trenchant blade; the pattern of wear in the more bulky posterior one is somewhat like a cross.

These combined characters amply justify the establishment of a separate family of Anthropoidea for the Malagasy fossil, intermediate in some respects between the South American Cebidæ and the Old-World Cercopithecidæ, besides presenting characters of its own.

Fam. nov. NESOPITHECIDÆ.

Gen. nov. *Nesopithecus Roberti.*

MEASUREMENTS.

	mm.
Length of dental series from canine to last molar (upper jaw) ...	61
,, upper molar series	23
,, premolar series	27
,, p_1	9
,, p_2	11·5
,, p_3	12·5
Transverse diameter of p_1, at basis	12
Length of upper canine	11·5
,, palate, in middle line	56
Width of skull, between zygomata	about 95
,, ,, between outer margins of posterior-premolar (p_1)	48·5
,, ,, ,, ,, canines	45·5
,, palate between inner margins of canines	29·5
,, ,, ,, ,, anterior molar (m_1) ...	24
,, interorbital spatium	24
Transverse diameter of orbit	about 25
Width of nasals above	12·5
,, ,, below	20
Lower jaw:	
Length of dental series exclusive of incisors	55·5
,, molar series	26·5
,, premolar series	28·5
Width of p_1	11
,, p_2	9
,, p_3	11
Width of lower incisor series between margins of outer alveoli ...	17·5
Height of horizontal ramus beneath m_1	26

The present discovery suggests the following general conclusions, which I expect will be corroborated by further finds.

(1) We may look forward in Continental Africa likewise for the discovery of Tertiary monkeys, intermediate between Cebidæ and Cercopithecidæ.

(2) The recent African Cercopithecidæ are not invaders from the north-east, as has been supposed; on the contrary, most, if not all, of the Tertiary monkeys of Europe and Asia are derived from the Ethiopian region. The home of a part at least of the Anthropoidea seems to have been in the Southern Hemisphere. This assumption is corroborated by the two facts—that Anthropoidea make their appearance suddenly for the first time in the later Tertiary of Europe and Asia, and that they are entirely absent from the Tertiary of North America.

II.—The Geological Work of the Conway Spitzbergen Expedition.

By E. J. Garwood, M.A., F.G.S., and J. W. Gregory, D.Sc., F.G.S.

THE discovery by Keilhau in 1827 of Carboniferous fossils in Spitzbergen first called attention to the geological interest of this archipelago. The collections subsequently made by Robert during the voyage of the "Recherche" in 1838, by Loven in 1837, by Drasche in 1872, and by the series of Swedish expeditions under Baron A. E. von Nordenskiöld between 1858 and 1873, proved the occurrence of beds belonging to the Archean, Lower Palæozoic, Devonian, Carboniferous, Triassic, Jurassic, Cretaceous, and Miocene systems. The fossils collected by these explorers are now in the museums of Christiania, Paris, Stockholm, and Vienna; but up till the present the only collections in London are a few specimens of Devonian fish obtained by exchange, and of Carboniferous Brachiopods collected by Mr. Lamont. It was, therefore, part of the work of Sir Martin Conway's expedition to obtain a series of fossils to represent as fully as possible all the successive faunas and floras of this far northern archipelago. Previous work on the geology of Spitzbergen had, moreover, been carried out on the coast, and it was Sir Martin Conway's main object to explore the interior. These two considerations lessened the thoroughness of the geological study of the country and the extent of the collections, for we had to travel long distances to reach rocks of different systems, and transport of heavy geological specimens from the interior was difficult. Nevertheless, considerable collections were made, which, besides serving to represent the Spitzbergen fossils, will probably aid in the more exact correlation of the horizons from which they come, and throw light on the migrations of the faunas.

The expedition arrived in Advent Bay on the 19th of June, and finished landing the stores by the evening of the next day. Preparations were at once made for the march inland, Sir Martin Conway and Mr. Garwood leaving on the 21st to establish the first food depôt. From this point the inland party pushed up the valley that ascends from the head of Advent Bay; after three days' march we camped on a col leading into a corresponding valley that runs inland from Sassen Bay. During the descent into this valley we found a remarkable esker, which told the story of its formation. At this point the party had to halt, owing to the collapse of the sledges; while one of us returned to Advent Bay to repair them, the other made a collection from the Triassic rocks of the mountains beside the camp. Below the Trias were beds of Carboniferous limestone and chert; a stream had cut a deep gorge through these, and at its head plunged over a fine waterfall, which gave our resting-place the name of "Waterfall Camp." The sledges having returned, we resumed our journey to the east. At first our route lay along a valley cut through the Triassic rocks to the Carboniferous; occasional outcrops of the latter could be seen on the foot of the valley-walls. Most of the floor was occupied by recent alluvium, while a series of raised

beaches containing shells of *Mya truncata* and *Saxicava arctica*, etc., formed terraces along the sides. Above the level of the marine terraces the valley contracted, and was blocked by a bar of moraine hills, 400 feet in height; in the hollows we often came upon beds of fossil ice, which added greatly to the difficulty of the traverse of these very irregular hills. After crossing the moraine, we had to leave the ponies and sledges and cross the glacier to the east coast, which we reached on July 16. We returned to Waterfall Camp by the same route; thence one of us walked back to Advent Bay overland, and the other along the coast, so that we were able to help in the geological examination of two east and west sections.

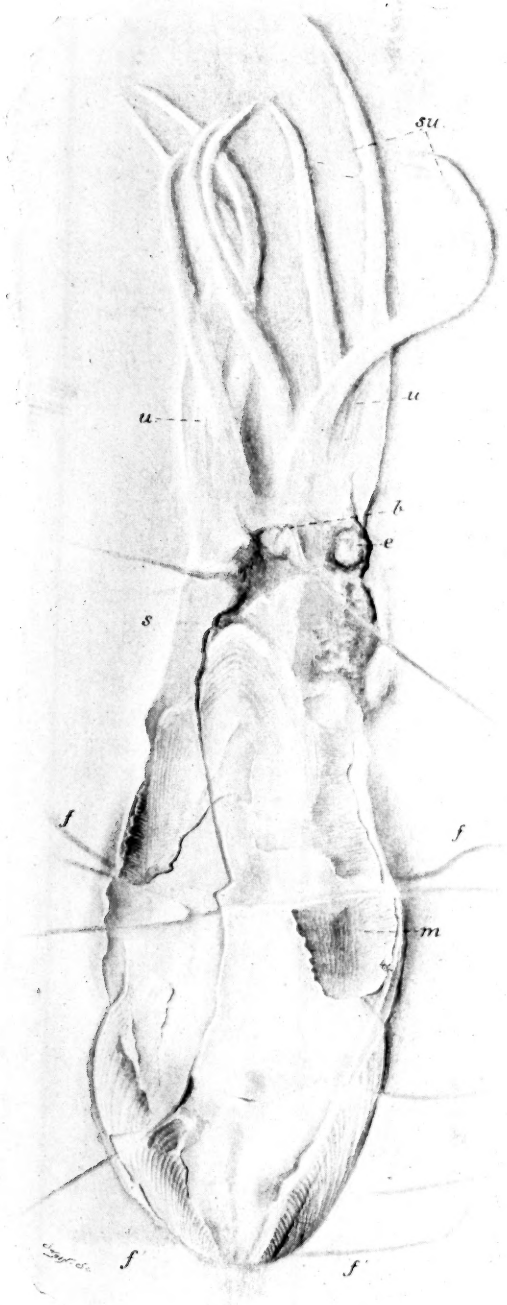
After meeting at Advent Bay we went with Mr. H. E. Conway, the artist of the expedition, to Green Harbour to collect fossil plants from the Tertiary beds there. Unfortunately the layer from which Baron Nordenskiöld made the collection in which Heer recognized over a hundred species, has been washed away by the sea. We obtained, however, a series of leaves of dicotyledons and of *Poacites*, and specimens of *Taxodium*, *Sequoia*, etc. Jurassic, Triassic, and Carboniferous rocks occur to the west of the Tertiary Plant-bed, and from these we also made collections.

After a few days' stay at this locality, Sir Martin Conway called for us in the "Expres," a small 13-ton steamer. In this vessel we sailed to the Seven Islands, through Hinlopen Straits, near to Prince Charles' Islands, and to the head of Wiide Bay. An attempt to circumnavigate Spitzbergen was frustrated by a belt of fast ice, which blocked up the passages into Stor Fiord, and formed a barrier across the broad Olga Strait. The excursion, however, gave us an opportunity for studying some fine sections in the Archean series of the Seven Islands, of the Devonian of Wiide Bay, and also of the Carboniferous and Tertiary rocks of Bel Sund, in which we spent a day during our return voyage.

One of us (E. J. G.) subsequently had the opportunity during the first ascent of Hornsund Tind, the highest mountain in Spitzbergen, of working out its geological structure.

It is too early to attempt any summary of the results of this journey, as many of the fossils are yet unpacked, and upon the examination of these the conclusions will depend. Sir Martin Conway made a careful map, on the scale of one inch to the mile, of the belt of country between Advent Bay and Agardh Bay, where we reached the east coast. We collected materials on which to prepare a geological edition of this map.

The stratigraphical sequence in Spitzbergen is remarkably complete, and its general characters are of great interest. The oldest beds are some schists, cut through and altered by some intrusive gneisses. These occur in the Seven Islands and in the north-west of Spitzbergen. Above these come the quartzites and schists of the Hecla Hook series; these are no doubt Lower Palæozoic, but fossils have not yet been found in them. The stratigraphical evidence proves them to be pre-Devonian. The series is named from their occurrence in the headland overlooking the bay in which Parry's ship, the "Hecla,"



G. M. Woodward, del.

Coccoteuthis hastiformis, Rüppell, sp.

Lower Kimeridgian (Lithographic Stone): Solenhofen, Bavaria.

spent the winter of 1826–7. Of the Devonian series we saw less than of the other formations, but obtained some fish-remains on the south-western shore of Wiide Bay. The Carboniferous system is one of the most extensively developed of any in Spitzbergen, and we obtained collections (mainly of Brachiopods) from various localities. The Carboniferous rocks are succeeded by those of the Trias, the two systems being sometimes separated by a slight unconformity. Another and greater break in the sequence occurs above the Trias, for the next fossiliferous beds apparently belong to the Upper Jurassic; fossils of this age occur in several localities, and they are succeeded by beds containing a species of *Aucella* very nearly allied, if not identical, with *A. concentrica*, Keys. After this comes another gap, including the upper part of the Cretaceous (if not the whole of that system) and the Lower Tertiary. There are no marine fossils later than the *Aucella* beds until we reach the Pleistocene raised beaches, with the exception of some unsatisfactory casts from beds associated with the Tertiary plant-beds.

The sequence of faunas in Spitzbergen is instructive, but the faunas themselves are thin. Species of certain classes, as of Brachiopods in the Carboniferous, and of Cephalopods in the Trias, may be numerous; but the classes represented are always very limited. The main lesson that the faunas teach seems to be that they always lived under unfavourable conditions. We had expected to find evidence of former climates not only much milder than the present, but even subtropical in warmth. In this we were unsuccessful; we found, on the contrary, beds of more than one period in the deposition of which ice must have taken part. We found boulders weighing three tons lying in beds of comparatively fine material; and it seems difficult to see what but ice could have transported them.

The ice of Spitzbergen consists of sea-ice, valley glaciers, and an ice-cap. The opportunity of the study of these three agents working side by side, was one of the greatest privileges we enjoyed during the expedition.

III.—On a Specimen of *Coccoteuthis hastiformis*, Rüppell, sp., from the Lithographic Stone, Solenhofen, Bavaria.

By G. C. Crick, F.G.S., of the British Museum (Natural History).

(PLATE XIV.)

THE specimen which forms the subject of the present notice belongs to the genus which is termed by some authors *Trachyteuthis*, and by others *Coccoteuthis*. Both names were originally given to the internal shell of a *Sepia*-like Cephalopod; the former to examples from the Lithographic Stone (Lower Kimeridgian) of Bavaria, and the latter to a specimen from the Kimeridge Clay of Dorset. They are regarded as synonymous, but some authors use one and some the other.

The genus was first figured in 1755 by Knorr,[1] who mistook the

[1] "Sammlung von Merkwürdigkeiten der Natur und Alterthümern des Erdbodens, welche petrificirte Körper enthält," pt. i, pl. xxii, fig. 2.

fossil for a fish. Under the name *Sepia prisca*, König [1] figured an example in 1825, but gave no description of the specimen. Rüppell,[2] however, in 1829 figured and described as *Sepia hastiformis* an example from the Lithographic Stone (Lower Kimeridgian) of Mühlheim, near Solenhofen, Bavaria. Figures of these shells were subsequently given by Münster,[3] Férussac and D'Orbigny,[4] and Quenstedt,[5] all of whom referred them to the genus *Sepia*.

H. von Meyer, in a letter in the Neues Jahrbuch for 1846 (p. 598), suggested the name *Trachyteuthis* for some *Sepia*-like internal shells from the Lithographic Stone of Solenhofen, but did not fully describe the specimens which he referred to this genus until October, 1855. In the meantime (May, 1855), however, Professor Owen [6] figured and described from the Kimeridge Clay of Dorset a similar shell, to which he gave the name *Coccoteuthis latipinnis*. Since there is no doubt as to the generic identity of these fossils, it appears that the name *Coccoteuthis* should be preserved, for it was the first to be adequately described; we have accordingly used it in the present paper, following Dr. A. Wagner,[7] who after full consideration of this question adopts Owen's name for the genus. Dr. K. A. von Zittel, however, uses the name *Trachyteuthis* both in his "Handbuch der Palæontologie" (vol. ii, p. 516) and also in his more recent "Grundzüge der Palæontologie" (p. 446).

So far as we have been able to ascertain, there have been but few references to the soft parts of the animal associated with the shell of this genus. Férussac and D'Orbigny figure [8] an example of the genus under the name *Sepia hastiformis*, and indicate anterior to the shell what may possibly be some remains of the animal. F. A. Quenstedt, in his "Cephalopoden" (pl. xxxii, fig. 1), figures an example of *Saepia hastiformis* with the impression of the lateral expansion of the mantle, which he says shows distinctly the transverse muscular striation. The shell associated with this impression is 170 mm. long. The same author, in the third edition of his "Handbuch der Petrefactenkunde," p. 508, pl. xxxix, fig. 8, describes and figures from the Lithographic Stone of Solenhofen the head of a Cephalopod (showing eight arms and the "beaks"), which he refers to *Loliginites priscus* (=*Plesioteuthis prisca*); but there does not appear to be any reason why the head should be associated with this species. It agrees very well with the head of the specimen described below, which undoubtedly belongs to the genus *Coccoteuthis*.

[1] "Icones Fossilium Sectiles," 1825, pl. xvii, fig. 201.

[2] "Abbildung und Beschreibung einiger neuen oder wenig gekannten Versteinerungen aus der Kalkschieferformation von Solenhofen."

[3] G. Münster, "Beiträge zur Petrefactenkunde," Heft vii, 1846, pl. ix.

[4] "Hist. nat. des Céphalopodes acetabulifères," 1835–48, *Sepia*, pls. xiv–xvi.

[5] "Petrefactenkunde Deutschlands," vol. i (Cephalopoden), 1846–9, pls. xxxi, xxxii.

[6] Quart. Journ. Geol. Soc., vol. xi (1855), pp. 124–5, pl. vii.

[7] Abhandl. d. k. Akad. d. Wissensch., Berlin, math.-phys. Cl., vol. viii, p. 754 *et seq.*

[8] "Hist. nat. des Céphalopodes acetabulifères," 1835–48, *Sepia*, pl. xvi, fig. 1.

In his "Handbuch der Palæontologie" (vol. ii, p. 516, 1884), Zittel states that distinct impressions of the animal occurring in the Lithographic Stone show the hinder part of the body to have been broad and sack-shaped, with convex sides, and the head to have had eight, rather long, similar arms. In his "Grundzüge der Palæontologie" (p. 446, 1895), the same author remarks that impressions of the sack-shaped body and of the head are sometimes preserved in the Lithographic Stone of Bavaria.

The British Museum has lately acquired a splendidly-preserved example of this genus (see Pl. XIV) from the Lithographic Stone (Lower Kimeridgian) of Solenhofen, in which the body with its lateral expansions, and the head with its arms, are all in union with each other. The specimen is displayed on the surface of a small slab, 210 mm. long and 100 mm. wide, and presents a dorsal aspect of the animal. The total length of the specimen from the posterior extremity of the body to the anterior extremity of the arms is 180 mm.

The body is somewhat flask-shaped, rounded posteriorly, and slightly contracted anteriorly. Its length is 94 mm.; its greatest width (which is at about the middle of the body) 43 mm.; whilst at the anterior boundary of the mantle, which coincides with the anterior extremity of the shell (*s*), the body is not more than 32 mm. wide. Fragments of the mantle still adhere both on the right- and on the left-hand sides of the dorsal surface, and these exhibit very clearly the transverse striation not infrequently seen in the fossilized state, as e.g. in specimens of *Plesioteuthis prisca* from the Lithographic Stone, and also in *Geoteuthis brevipinnis* and *Belemnoteuthis antiqua* from the Oxford Clay of Wiltshire. A fragment (*m*) on the right-hand side of the dorsal surface extends from the lateral boundary of the body almost to the median line of this surface. Its lateral portion is of a very pale reddish-brown tint, but this colour becomes more intense towards the middle of the dorsal surface, where it is of a rich reddish-brown colour. This does not appear to be a stain, but is probably due to the original colour of the mantle. The lateral portion shows the usual transverse striation, but the median portion bears also longitudinal wrinkles.

The body was provided with lateral expansions or fins, as seen on both the right- and left-hand sides of the specimen. These seem to have been divided into a large anterior portion and a much smaller posterior part. That on the right-hand side is the more nearly complete; its posterior portion is trapezoidal in outline, antero-posteriorly it measures 9 mm., its width at the middle being 19 mm. Its posterior boundary (*f'*) is very slightly waved and nearly at right angles to the median line of the body. It arises from the body at about 3 mm. in advance of the posterior end; and at a distance of about 20·5 mm. from the boundary of the body it curves abruptly forward, and passes almost at right angles to its previous direction to meet the posterior boundary of the anterior larger portion of the lateral expansion. This posterior boundary of the anterior portion cannot be made out close to the general outline

of the body, so that the anterior part appears to have been possibly for a width of about 5 mm. continuous with the posterior portion. This boundary curves gently outwards and upwards to the edge of the slab containing the fossil. The width of the expansion that is preserved is about 37 mm.; it is fairly smooth, with very obscure transverse striation, the rest of the surface of the slab being somewhat roughened and also of a lighter colour; its outer boundary was removed in reducing the specimen before it came into the National Collection. The anterior boundary (*f*) seems to be indicated by a faint line of colour arising from the margin of the body at a point 56·5 mm. from the posterior end; it curves outwards and rather abruptly forwards, and can be traced for about 17 mm. The antero-posterior length of this expansion at the margin of the body is 44 mm.

On the left-hand side of the specimen a portion of the lateral expansion is also shown, but unfortunately a much greater part than on the right-hand side has been removed in reducing the slab. The posterior portion has about the same antero-posterior dimension as on the other side, but appears to have been only about 12·5 mm. wide; its posterior boundary (*f'*), which is well preserved, is slightly curved and nearly at right angles to the median line of the body. On this side, too, the posterior boundary of the anterior larger portion cannot be traced to the margin of the body, and appears to support the idea that the two portions of the lateral expansion were continuous for a short distance from the margin of the body. The posterior boundary[1] of the larger portion curves like that on the opposite side of the body, but is intercepted by the edge of the slab at a width of about 16 mm. The anterior boundary (*f*) arises at about 57·5 mm. from the posterior extremity of the body, passes outwards and upwards to the edge of the slab at an angle of about 45° with the median line of the body.

Only portions of the internal shell are preserved, viz., the anterior portion, part of one of the sides, the greater part of one of the wing-like expansions, and the impression of the other expansion, but these are quite sufficient to indicate the complete outline of the shell. It is lanceolate-oval in form, 95 mm. long and 29 mm. in width, excluding the lateral expansions. The hinder three-sevenths (about 40 mm.) is provided with wing-like expansions; its greatest width, including these, is 41 mm. at about 28 mm. from the posterior extremity. There is no trace of the ink-bag. The head, with the arms, has a length of 85 mm. The greatest width of the head is 31 mm. On the right-hand side there is a mass of calcite, a portion of which possibly occupies what was the orbital cavity (*e*). About in the centre of the head there is a triangular indentation (*b*), having its apex directed forward and a little to the right side; this is without doubt the impression of one of the mandibles or "beaks."

Eight arms can be made out; they do not differ greatly in size. There appears to be no trace of the tentacular arms, but such may

[1] This is scarcely visible in the figure.

nevertheless have been present, for in the living *Sepia officinalis* "the tentacular arms remain contracted within the others when in repose."[1] The two dorsal arms are about 70 mm. long and 6 mm. wide at their base; for a short distance they taper rather rapidly, then much more gradually to their extremities. The next pair is a little longer and a little more robust. The third pair is about the same as the first, and the fourth or ventral pair a little shorter and less robust than the first. A marked irregularity in the outline of the inner surface of the arms is doubtless due to the presence of small acetabula or "suckers," but there are no indications of hooks. The first and second arms on either side seem to have been united for some short extent (perhaps one-third of their length) by a web-like membrane, which probably also connected some of the other arms, but this appears not to have united the bases of the two dorsal arms.

Dr. A. Wagner, in his memoir already alluded to, groups all the examples of *Coccoteuthis* which have been described from the Lithographic Stone into one species, viz. *C. hastiformis*, recognizing amongst them three varieties—(i) var. *minor*, (ii) var. *media*, and (iii) var. *maxima*. Fortunately in the specimen described above, the shell is preserved so that it can be readily compared with the species which have already been described. It is referable to Wagner's var. *minor*.

EXPLANATION OF PLATE XIV.

su. The first (dorsal) and second arms of the right side, showing acetabula or "suckers" on their inner surfaces.
u. Membrane uniting the bases of the first (dorsal) and second arms on either side.
b. Impression of beak or mandible.
e. Probable position of eye.
s. Anterior extremity of shell.
m. Portion of mantle, exhibiting wrinkles and traces of colour.
f. Anterior boundary of lateral fin on either side.
f'. Posterior boundary of same.

Slightly reduced from the original.

IV.—The Search for *Uintacrinus* in England and Westphalia.

By F. A. Bather, M.A.,
Assistant in the British Museum (Natural History).

IT is just a score of years since the unstalked crinoid *Uintacrinus* was discovered, almost simultaneously, in the Niobrara Chalk of Kansas and the Lower Senonian of Westphalia. The American specimens were described by Grinnell and Meek, while the single European specimen was exhaustively discussed by Schlueter. Of recent years further specimens, in a better state of preservation, have been found in Kansas, and a slab purchased by the British Museum enabled me to make a more detailed study, the results of which were published in the Proceedings of the Zoological Society (vol. 1893, pp. 974–1004, pls. liv–lvi, April, 1896). But as to

[1] Tryon, "Manual of Conchology," vol. i, 1879, p. 58.

the occurrence of *Uintacrinus* in Europe, our knowledge at the beginning of 1896 remained as in 1876; not even from Recklinghausen, the original Westphalian locality, had another fragment been obtained. It may therefore be a surprise to many to learn that *Uintacrinus* is one of the commonest fossils of the *Marsupites* zone, not only in the Marlstone of Westphalia, but in the Chalk of our own island, and probably at the same horizon in a good many other countries.

It was in March last that Mr. C. D. Sherborn brought to me from Dr. A. W. Rowe, of Margate, a few scattered plates, supposed to belong to a small *Marsupites*. I saw at once that they belonged to *Uintacrinus*, though the species was uncertain; and having obtained two days' leave for the purpose, I set off for Margate, on Dr. Rowe's invitation, to examine his collection and to search the cliffs. Dr. Rowe has several fragments showing the plates in juxtaposition, and these fully confirmed my opinion. Moreover, a search, with the help of Mr. Sherborn, Dr. Rowe, and my wife, showed that along the cliffs east of Margate, next to columnals of *Bourgueticrinus*, cup-plates and brachials of *Uintacrinus* were the commonest fossils. We did not find it in the same places as *Marsupites* and *Echinocorys scutatus*, although Dr. Rowe says that he has found it in various localities west of Margate, associated with *Marsupites*. In any case it occurs at the horizon of *Actinocamax verus*.

Another diligent collector, Mr. C. Griffith, of Winchester College, showed me, last July, plates from the railway-cutting west of Grately station, near Andover, and from a pit one mile north-east of Wherwell railway station, which undoubtedly belonged to *Uintacrinus*. Among the associated fossils were said to be *Bourgueticrinus*, *Actinocamax verus*, *Micraster cor-anguinum*, and *Echinocorys*.

Clearly, then, *Uintacrinus* was common enough in England; but the fragments had, for the most part, been disdained by collectors, or relegated to "Marsupites?" Thus, in the Wetherell Collection at the British Museum there are *Uintacrinus* plates from the north of Kent, which had been labelled *Marsupites*, and which, when rapidly sorting the collection some eight years ago, I had set aside as belonging to an undetermined genus.

These discoveries suggested that *Uintacrinus* could not be so rare at Recklinghausen; so when the holidays came, off we went thither, visiting Bonn, Professor Schlueter, and the type-specimen on the way.

It is not easy to collect fossils at Recklinghausen. The rock is a glauconiferous sandstone, the grains of which are cemented by carbonate of lime, and coloured a greenish-gray by carbonate of iron. When freshly quarried it is a hard, massive stone; but exposure to the atmosphere rapidly dissolves the lime and decomposes the iron carbonate, leaving eventually a loose sand, coloured reddish-brown or yellow by iron peroxide. The rock is therefore useless for building purposes and for road-metal, and is not quarried in a regular manner, although there are a few pits in some unfossiliferous sand that seems to have arisen from the decomposition and perhaps

partial erosion of the hard rock. Fortunately the soil requires occasional replenishing with lime, and this is supplied by scattering over the fields lumps of the fresh rock, such as may be obtainable from any casual openings. Such lumps and openings are the collector's only hope.

I reserve a statement of the various localities searched by us, and the fossils there found, for a future detailed description of the European specimens of *Uintacrinus*. The main result is that at Recklinghausen, as at Margate, *Uintacrinus* is, next to *Bourgueticrinus*, the commonest fossil. It occurs, associated with *Actinocamax verus* and *A. quadratus*; but, as at Margate, we found no *Marsupites*. The greater number of the remains consist of scattered cup-plates and brachials, differing only in colour from those so common in the Margate Chalk. This was all that I expected to find, so that I was all the more delighted when my wife discovered at the junction of the humus and the sand, in a small roadside cutting east of the railway station, a complete cup of *Uintacrinus*, less perfect than the type-specimen as regards the arms, but more perfect in its base and the important interbrachial areas. The specimen was extracted with great care, delicately laid in a round tin box, and packed in with fine dry sand. Thus it travelled safely until its loose substance could be hardened with water-glass.

The object of this note is to draw the attention of those who collect Upper Cretaceous fossils to this abundant species, which has been so strangely overlooked. If they have not already in their collections, they will doubtless find in the Lower Senonian of their neighbourhood, flattish, thin plates, usually of pentagonal or tetragonal outline, and marked on the inner surface with wide grooves radiating from the centre to the sides, not to the angles. Associated with these they will find brachials, often characterized by a diagonal fulcral ridge, as figured in the paper above referred to. I shall have pleasure in sending a copy of that paper to anyone who will look out for *Uintacrinus*, and will lend me the specimens he finds.

Possibly with this assistance many important questions may be solved. We want to know, first, if there are really two species of *Uintacrinus*, as has been supposed; and if both are represented in Europe; or if the European specimens belong to the American species: this can only be decided by the comparison of many specimens. Secondly, we have to determine the geological and geographical range of *Uintacrinus* in Europe: for this, details of its occurrence, and especially of the associated fossils, are desired. Does it really belong to the *Marsupites* zone? Thirdly, when this knowledge is obtained, we may be able to throw light on the correlation of the Cretaceous rocks in America and Europe, and to distinguish further between synchronism and homotaxis. Other questions there are of morphological and bionomic interest; but I hope enough has already been said to kindle in the breasts of geologists some enthusiasm for *Uintacrinus*.

V.—Recent Discoveries of Fossil Plants in Argentina. (Extract of a Letter from Dr. F. Kurtz, National Academy of Sciences, Cordoba, Argentine Republic.)

Communicated by W. T. Blanford.

[The following is an extract from a letter just received from Dr. Kurtz, who first discovered and described a Lower Gondwána (*Glossopteris*) flora in Argentina about two years ago. His paper was published in Spanish in the Revista del Museo de La Plata, vol. vi, and a translation in English was inserted by Mr. Griesbach, Director of the Geological Survey of India, in the Records of the Survey, vol. xxviii, 1895, pt. 3, p. 111. Shortly afterwards, Professor Zeiller added some very important and remarkable facts showing the occurrence of the same flora, associated with *Lepidodendron*, in Southern Brazil; and I wrote, at Mr. Griesbach's request, a brief comment on the data so far accumulated for the Records of the Geological Survey of India, in the May number of which, for the present year, my note was published. Dr. Kurtz's letter, containing several very interesting additions to our knowledge of the Argentine fossil floras, was written on the receipt of my paper, and as he has kindly allowed me to make the facts known, I have much pleasure in doing so, by permission of the Editor of the Geological Magazine.—W. T. B.]

THE most important addition to the fossil flora of the Bajo de Velis (which locality I visited from Dec., 1894, to March, 1895) is the discovery of *Rhipidopsis gingkoides*, Schmalh., and *R. densinervis*, Fstm., each represented by well-preserved leaves and numerous fruits. Both species are met with in the Damudas of India—*R. densinervis* in the Rániganj (Kámthi group), *R. gingkoides* in the Barákar (Duranga Coalfield), the latter together with *Cyclopitys dichotoma*, Fstm. (this curious type was detected by my friend and colleague, Dr. Bodenbender, in the Sierra de Los Llanos, in the south of the province of La Rioja).

Schmalhausen described his *Rhipidopsis gingkoides* from deposits at the Petschora, which he considered to be of Jurassic age, but which will be more properly associated with the Permian, as already pointed out by Mr. C. Kosmovsky.[1] [I am almost convinced that the splendid *Zamiopteris glossopteroides*, Schmalh. (Mem. Ac. Sci. Petersbg., ser. vii, vol. xxvii, No. 4, pl. xiv, 1–3), from the Lower Tunguska, occurring there with *Nöggerathiopsis* (= *Rhiptozamites*, Schmalh.), is a true *Glossopteris*.] Besides the two species of *Rhipidopsis*, I found splendid and complete specimens of *Equisetites Morenianus*, Kurtz, proving this plant to belong to one of the larger species of *Equisetites* (the joints of the stem being 3 cm. long and nearly as broad).

During various expeditions to the Sierra de Los Llanos, Dr.

[1] C. Kosmovsky, "Quelques mots sur les couches à végétaux fossiles dans la Russie orientale et dans la Sibérie": Bull. Soc. Imp. Nat. de Moscou, 1891, No. 1, pp. 170–7.

Bodenbender collected the following fossil plants (besides some doubtful ones) :—

Neuropteridium validum, Fstm. (abundant).
Glossopteris communis, Fstm., or an allied form (the first *Glossopteris* in America; rather common).
——— *retifera*, Fstm. ! (rare).
Phyllotheca, sp. (common).
Lepidodendron Pedroanum (Carruthers), Szajnocha.[1]
——— *Sternbergii*, Brong. (together with *Nöggerathiopsis Hislopi*, Fstm.).
Cyclopitys dichotoma, Fstm.

Near Carizal, in the Sierra Famatina, province of La Rioja, Bodenbender met with a splendid fern—*Sphenopteris*, sp. nov. (*ex aff. S. flexibilis*, Heer)—and *Phyllotheca*, sp.

Furthermore, at Trapiche, near Guandacol, in the same province, he detected a beautiful *Lepidodendron* (stem and leaves, but the leaf-scars unfortunately not well preserved), which I take to be *Lepidophloios laricinus*, Sternb. (also found in Rio Grande do Sul with *Gangamopteris cyclopteroides*, Fstm., var. *attenuata*, Fstm.), together with *Neuropteridium validum*, Fstm., the *Lepidophloios* occurring in strata *overlying* that containing *Neuropteridium*.

My friend Dr. Bodenbender regards all these deposits of Bajo de Velis, Pampa de Anzalan (= Sierra de Los Llanos), Sierra de La Rioja (Vilgo, Amanao, Saladillas), Carizal, and Trapiche,[2] as belonging to the same horizon, which he calls Permo-Carboniferous, only making a stratigraphical difference between Carizal with Trapiche on one side and all the other localities on the other side. This is a geological question with which I have not to deal, but, speaking in a purely botanical sense, I should discern at least two local horizons, the first represented by the Bajo de Velis alone, the other by the rest. The differences and similarities of these two groups are as follows :—

1. Common to both are *Neuropteridium validum* (very scarce in the Bajo de Velis, abundant in the Sierra de Los Llanos, scarce at Trapiche) and *Nöggerathiopsis Hislopi* (abundant in the Bajo de Velis, very common in the Sierra de Los Llanos).

2. At the Bajo de Velis alone were found: *Gangamopteris cyclopteroides* (not very frequently), *Equisetites Morenianus* (not very frequently), *Euryphyllum Whittyanum*, Fstm. (one leaf, which I described as a variety of *Nöggerathiopsis*, but which Zeiller, *l.c.*, thinks may be *Euryphyllum*), and the two *Rhipidopsis*. On the other hand, we know only from the Sierra de Los Llanos, etc., the two species of *Glossopteris*, the *Phyllothecas* (abundant), the *Lepidodendrons*, and the *Cyclopitys*. There remain the *Sphenopteris* and the

[1] The plant from Retavisto, in the province of San Juan, which Szajnocha (Sitzungsber. Ak. Wiss. Wien, Bd. C, Abth. i, p. 5) considered to be this species, belongs rather to a type of the Culm, perhaps to *L. Volkmanni*, Sternb. (R. Zeiller, Bull. Soc Geol. France, J. xxiii, 1895, p. 608.)

[2] He is rather inclined even to include Retavisto, which I take to be Lower Carboniferous (Culm).

Lepidophloios of the northern deposits, distinguished stratigraphically by Bodenbender (who will put forth his views in two memoirs—one in our Boletin, with a number of sections; the other in German accompanying a paper by Professor E. Kayser, describing the Devonian fossils collected by Bodenbender). The little diagram (Table A) at the end will perhaps explain the state of the flora better than my description.

From Cacheuta, in the province of Mendoza, we have received two beautiful collections of fossil plants made under the direction of Mr. E. Glaser, formerly Director of Petroleum Mines at that place. We had also smaller collections made by Dr. L. Brachebusch and by Dr. Bodenbender.

The principal species of this deposit are :—

Danœa, sp. nov. (a splendid fern).
Asplenium Whitbyense (Brong.), Heer (not rare).
Sphenopteris elongata, Carruthers (rather common).
Thinnfeldia odontopteroides (Morris), Fstm. (abundant).[1]
——— *lancifolia* (Morris), Szajn. (abundant).
Bravardia Mendozensis, Hanthal (perhaps the fruit-bearing state of a *Thinnfeldia*, the only indusia visible quite resembling those of *Aspidium*, sect. *Polystichum*).
Pecopteris tenuis, Schouw.
Oleandridium Mareyesiacum (Gein.), Kurtz (not rare).
——— sp. nov. (common).
Podozamites elongatus (Morris), Fstm., var. *latior*, Fstm. (common).
Zamites, sp. nov.
Sphenozamites, sp. nov.
Pterophyllum, sp. nov. (*ex aff. P. Carnalliani*, Göpp., rather common).
Baiera, sp. nov. (common).
Cardiopteris Juberi, Szajn., justly removed from this old Culm genus by Nathorst (*Ptilozamites*, Z. Nath.), proves to be nothing but a little curious form of *Thinnfeldia lancifolia*, Szajnocha. As you remarked in the "Records," the character of the Cacheuta flora is Australian and African, and has hardly anything in common with the higher Indian Gondwána floras.

The last discovery of fossil plants, made by Dr. Jose A. Salas, of Mendoza, a very zealous examiner of coal-mines in the Cordillera, furnished me with a small collection of types, derived from the mines of "Del Transito" on the Rio Atuel (southern part of the province of Mendoza), which prove the existence there of something like a Rajmahál flora. The plants I have been able to determine up to the present are as follows (in the same region were found Liassic animals) :—

Asplenium Whitbyense (Brong.), Heer.
Macroteniopteris, sp.

[1] The *Thinnfeldia odontopteroides* of the Upper Gondwánas is—as far as one may judge from the drawings in the Gondwána Flora (Pal. Ind.)—rather a doubtful form.

Oleandridium vittatum (Brong.), Schimper (?).
Pterophyllum princeps, Oldham & Morris (? *P. Morrisianum*, Oldh., if these two are distinct species!).
Pterophyllum Rajmahalense, Morris.
Palæozamia cf. *brevifolia*, Braun.
Ptilophyllum, sp.
Walchia, sp.

Finally, I should like to call your attention to the papers of my friend H. von Jhering (now Director of the Museum Paulista in São Paulo, Brazil) bearing upon the old Mesozoic relations of South America with New Zealand and Australia—"Die geographische Verbeitung der Flussmuscheln" (Das Ausland, 1890, Nos. 48 and 49) and "Ueber die alten Beziehungen zwischen Neuseeland und Sudamerika" (*ibid.*, 1891, No. 18)—if you are not already acquainted with them.

TABLE (A) REFERRED TO ABOVE (see p. 448).

Bajo de Velis.	Sierra de Los Llanos, Sierra de la Rioja (Vilgo, Amanao).
Neuropteridium validum, Fstm.	
	Glossopteris communis, Fstm. (?).
	——— *retifera*, Fstm.
Gangamopteris cyclopteroides, Fstm.	
	Phyllotheca.
Equisetites Morenianus, Kurtz.	
	Lepidodendron Pedroanum, Szajn.
	——— *Sternbergi*, Brong.
Nöggerathiopsis Hislopi, Fstm.	
? *Euryphyllum Whittianum*, Fstm. (?).	
Rhipidopsis gingkoides, Schmalh.	
——— *densinervis*, Fstm.	
	Cyclopitys dichotoma, Fstm.

Academia Nacional de Ciencias. Cordoba (Rep. Argent.), 5 Aug. 1896. Dr. F. KURTZ.

VI.—THE CHALKY AND OTHER POST-TERTIARY CLAYS OF EASTERN ENGLAND.

By Sir HENRY H. HOWORTH, K.C.I.E., M.P., F.R.S., F.G.S.

AMONG the so-called Glacial beds, none fills a larger place in geological literature than the Chalky Clay of Eastern England. I prefer to call it the Chalky Clay, as Searles Wood named it, rather than the Chalky Boulder-clay, because boulders in the true sense of the word, such as characterize the genuine Boulder-clays of North Britain, are infrequent in it. The term Chalky applied to this clay depends on the fact that it is more or less crowded with chalk rubble and chalk fragments of various sizes, and that it has also incorporated in it a considerable quantity of chalk dust, whence

its colour and superficial appearance. These peculiarities, which mark it over a wide area from Yorkshire to Finchley, and from Southwold to Warwickshire, are, nevertheless, a secondary, and not a primary, feature of the clay, and have disguised and confused the problem of its explanation.

It has been noticed by several writers that, while there is a common appearance to this clay wherever found, due to the fact that it contains much débris of chalk strata, yet that in regard to its other contents, and notably its matrix, it varies in accordance with the composition of the beds over which it lies, that is, with the substratum. This fact has been frequently noticed, and was, so far as I know, first observed by the Rev. W. B. Clarke, who, writing as far back as 1837, says:—"The diluvial clay covers a great portion of Suffolk, Norfolk, Cambridgeshire, and Essex, and at Cromer rises to 400 feet; much of it is yellowish, but the greater part blue. In both cases it contains chalk pebbles, sometimes in layers, but generally dispersed. This at once distinguishes it from the London and Plastic Clays." Mr. Clarke then goes on to argue that "the yellow clay was derived from the plastic, and the blue, from its peculiar fossils, from the clay below the Chalk" (Geol. Transactions, ser. II, v, p. 365).

These observations of a very good geologist have been amply confirmed by later explorers. Thus Mr. Skertchly says, speaking of the Chalk, the Kimeridge and Oxford Clays:—"We find that the Boulder-clay lying upon these rocks partakes of their physical character. Thus, upon the Chalk the Boulder-clay is very chalky, and, indeed, in some places, as at Mareham le Fen in Lincolnshire, and Thetford in Norfolk, it is almost entirely made up of that substance; at the former place it is quarried and burnt for lime, and at the latter the presence of seams of clay and ice-scratched flints alone enables us to discriminate between it and the chalk beneath. The Kimmeridge Clay is darker than the Oxford Clay, and we accordingly find the Boulder-clay which reposes upon the former is darker than that which lies upon the latter. Where boulders are rare, it is sometimes very difficult to distinguish the Boulder-clay from the older rocks." ("Great Ice Age," new ed., p. 346.) "The Gault clay again takes the ground in but a small area in the Fens, but the Boulder-clay 'picks it out' as it were, and at Modney Bridge brickyard, near Hilgay, for example, I have known the glacial bed to be mistaken for Gault by persons quite familiar with the latter." "Similar remarks," says Mr. Skertchly, "apply to all other formations upon which I have mapped Boulder-clay. For example, the light-blue Upper Lias Clay of Leicestershire impresses its character upon the Boulder-clay which overlies it, and the other members of the Liassic group, where they are in force, behave in a similar manner. The Great Lincolnshire (Inferior) Oolite Limestone around Melton Mowbray yields so large a quantity of material to the Boulder-clay there, that I have been in doubt as to whether the deposit might not be faulted limestone. These peculiarities are at once and correctly expressed by the statement

that the ingredients of the Boulder-clay are, for the most part, supplied by rocks, upon or near which it reposes. That this is actually the case, and not an accident of colour, is further attested by the included fossils; *Gryphæa dilatata* and *Belemnites Owenii*, for example, are abundant upon the Oxford Clay, and *Ostræa deltoidea* upon the Kimmeridge Clay." (Skertchly, communication to Geikie's "Great Ice Age," new ed., p. 346.)

It is curious that those writers who have been most ready to concede this change in the Chalky Clay, in accordance with its substratum, have not gone a step further, and also seen that the chalky character itself of the clay in certain districts is simply due to its being upon, or in close proximity to, the Chalk *in sitû*, and that there is no justification for constituting the Chalky Clay a separate horizon. It is because the Chalk occupies so much of the area, and is itself so easily disintegrated, that the name Chalky Clay, rather than Oolitic Clay or Liassic Clay, has been not improperly given to it.

That this feature of the Chalky Clay is due entirely to its lying upon or close to chalk, is also proved by another feature of its distribution, which is interesting for more than one reason, and which has not been sufficiently noticed. In the first place, the Chalky Clay is, so far as I know, largely confined to Eastern England, and is found nowhere else in the world, pointing to there not being the actual conditions elsewhere which prevail here. But this is not all. Mr. Searles Wood, jun., once published a little map roughly defining the area in which it occurs. A larger and more detailed map is appended to a manuscript memoir of his in the possession of the Geological Society, but it has only been since the detailed plotting and mapping of this area by the Geological Survey that it has been possible to define its frontiers, and it would be very useful to us all if the results thus obtained were set out on a map of moderate dimensions.

It will be seen that the distribution of the Chalky Clay, when thus viewed as a whole, is very remarkable. In the first place, it is entirely an inland deposit with duly circumscribed limits and boundaries. It really occurs in several detached masses—two of them in Lincolnshire, one in Yorkshire of no great importance, and a fourth covering more or less an area of several hundred square miles round the depression of the Fens.

In each case the area occupied by the Clay is an insular area, separated from the sea by other surface beds. At one point only—and this is clearly accidental, and due to the recent cutting back of the coast—does the Chalky Clay look down on the sea. In Lincolnshire it is only found to the west of the Lincolnshire Wolds; while in its great homeland further south it is separated from the sea on all sides by beds of Crag and of so-called contorted beds—Middle Glacial beds, etc. This is the case all round the coasts of Norfolk, Suffolk, and Essex, and in the low-lying northern frontier of the marshes and peat-lands of the Wash. To the south it thins out, and virtually ends with the hills bordering the Thames

on the north. In the west its limits have not been quite defined, but it occurs abundantly in Leicestershire and Rutland, in Bedfordshire and Buckinghamshire, in Nottinghamshire, and parts of Staffordshire and Warwickshire; on all sides it is limited, however, by other beds, and forms a great concentric ribbon or ring round the Fen country.

The lesson I wish to deduce from these facts is, that the chalky nature of this clay in certain places is no criterion of a separate origin and a separate history for the deposit. It means no more than that the same clay, where it lies on or near chalk, is chalky; where it lies on or near Oolitic beds, is largely Oolitic; and where it lies on or near Liassic beds, is Liassic. Where, again, it is remote from chalk, chalk débris is necessarily not present in it, and yet the clays may be, and probably are, of the same age, produced by the same forces, and differentiated from each other only as the sandy deposits of one part of a bay are differentiated from the muddy deposits of another part of it;—the fact is that the Chalky Clay, which, in the eyes of so many geologists, forms a deposit which is treated as *sui generis*, and as marking a particular horizon, is nothing of the kind, but, as I believe, is merely a local form of other clays occurring in Eastern England, which do not contain chalk débris, but which resemble it in other respects, and which are, so far as we know, interlocked with it or mark the same horizon. This very fact, however, involves an issue of importance; for it may well be that where the chalk débris was not available for incorporation in the clay, the clay itself may be of precisely the same age, and be otherwise continuous with the Chalky Clay; and that instead of there being several superficial clays in Eastern England, whose various names, such as Stony Clay, or Hessle Clay, or Purple Clay, etc., suggest a varying origin, there may be only one such clay, marked in different areas by necessarily different characters, pointing, not to a different date, but to different ingredients, and perhaps a different provenance.

Two Boulder-clays have been described from Norfolk—the Upper or Chalky Boulder-clay and the stony loam or Lower Boulder-clay. The difficulty of separating the two may be judged from the following sentence of Mr. H. B. Woodward. He says:—"So little brick-earth is met with that for a long time I could not settle in my mind whether or not the Chalky Boulder-clay was distinct from the stony loam. The absence or rarity of this formation where the Lower Glacial brick-earth was well developed seemed to favour the notion. The apparent passage of stony loam into chalky loam at the brickyard in Long John's Road, also in the railway-cuttings north-west of Hapton; the difficulty in the parishes of Postwick, Brundall, and Plumstead of drawing a line between the Chalky Boulder-clay and the stony loam, where the two seem from their physical relations to merge one into the other, tended to support the supposition that they were but one formation." Mr. Woodward quotes the pits at Upton Hamlington and South Walsham as throwing some light on the subject

("Geol. of Norwich," p. 103). This light, it must be admitted, is a very small one, and he confesses that it illustrates the uncertainty of their development. Both beds contain chalk rubble; in some cases patches of very chalky clay appear in the stony loam; and towards Cromer, and further west, near Weybourn, these marly beds are worked for lime. Sometimes the beds in this stony loam are well stratified and laminated, and often inland, as well as on the coast, exhibit many and remarkable contortions. Hence the term contorted drift applied by Lyell. These contortions include masses or galls of sand and gravel.

One feature, supposed to distinguish the Chalky Clay from the stony loam, is the absence of shell fragments, but, as Mr. Woodward says—"It is by no means improbable that the stronger shells of the Crag period, such as *Cyprina Islandica*, might be caught up and preserved in it, as well as Liassic and other older fossils, or as the shells of the Kimeridge and Oxford Clays. And, indeed, shell fragments have been noticed by Messrs. Bennett, Blake, Skertchly, Reid, and myself, in the Boulder-clay at Flordon, and near Rockland St. Mary" (*id.*, p. 115). Mr. Woodward himself confesses that at Burlingham the Chalky Boulder-clay is much obscured by a loamy soil, so that it is difficult to distinguish it from the Lower Glacial brick-earth (*id.*, p. 119), which is surely an inversion of matters if the distinction is to be maintained.

Let us now turn elsewhere. The Lincolnshire Wolds, as is well known, separate the Chalky Clay of Lincolnshire from what I may call the maritime clays. Here, again, it is difficult to assign a different horizon to the two. It was remarked long ago that the clay in Lincolnshire is often without chalk where remote from the Chalk Wolds (Geol. Mem. N. Linc. and S. Yorkshire). While Mr. Jukes-Browne separates the Boulder-clays of South-west Lincolnshire into an older and younger Boulder-clay, he says some of his colleagues who had worked in that and adjoining areas regard the Boulder-clays as approximately of the same age (Mem. S.W. Linc., p. 74).

"In East Lincolnshire," says Mr. Jukes-Browne, "there are only three localities where the brown Boulder-clay comes in contact with the white Boulder-clay"; and he concludes, after examining them, that the appearances at those places are not against the supposition that the brown clays pass into the Chalky Clay. Mr. Bulman says the separation of the Chalky Clay and the Purple Clays of the Eastern Counties seems to have been made on arbitrary grounds. They do not occur in the same district, being separated by the Wolds. (GEOL. MAG. 1891, p. 345.) Mr. Jukes-Browne similarly shows that the so-called Purple Clay and the Hessle Clay graduate into each other, and that there is no break between them (*id.*). Mr. D. Mackintosh speaks of the Purple Clay of East Yorkshire as horizontally continuous with the Chalky Clay of Lincolnshire (Q.J.G.S., vol. xxxvi, p. 187).

Young and Bird long ago discriminated the three Boulder-clays of Yorkshire, which are superficially marked by certain characters,

but which they admit, as do more recent explorers, pass into each other. Thus they say of the most distinct of the three, the so-called basement clay—a bluish or blackish, tenacious clay, forming the lowest visible portion of the cliffs in Holderness and elsewhere:—"As the brown clay passes into the ash-coloured, so the latter passes into the blue clay, which often occurs in patches rather than a distinct bed. Indeed, all the three kinds of clay are often seen banded together in one mass, but we generally find the brown clay uppermost, the ash-coloured in the middle, and the blue, tenacious clay in the lowest place." (*Op. cit.*, pp. 17, 18.)

Mr. C. Reid, who is disposed to postulate two Boulder-clays in Holderness, separated by gravel, in some cases by fossiliferous gravel, says: "It is also interesting to find that this Boulder-clay" [underlying the fossiliferous gravel in one pit] "is, in its lithological character, quite indistinguishable from the newer chalky and purple Boulder-clay which overlies the gravel further east." This is in the valley of Croxton. He also mentions how, south of Laceby, "the gravels suddenly thin out and the two Boulder-clays come together."

At Grimsby the sections "showed two Boulder-clays, purple and chalky, and exactly alike, separated sometimes by a mere line of division, sometimes by gravelly sand, in which fragments of inter-Glacial shells were found. Though the sections were examined almost daily, not the slightest difference could be detected between the two Boulder-clays, either in their matrix or their included boulders." (C. Reid, Linc. and Yorks. Surv. Mem., pp. 169, 170.)

This will suffice to show that, essentially, the Boulder-clays of Eastern England are of one age, and graduate into each other. Their peculiarities are local, due to local causes, and mark only superficial *differentiæ*, and do not justify their being assigned to different periods. Their difference of contents depends very largely on their covering a different kind of substratum. The element and factor which unites them is the presence in them all of a certain number of foreign stones and débris, having, apparently, a common origin, and pointing to a common explanation.

Let us now turn to the contents of the clays, and especially of the Chalky Clay. The first ingredient of these clays which is noticeable is the clay itself, and I cannot help remarking that it is a pity the Geological Surveyors, in the various memoirs they have given us on the so-called Glacial beds, have not given us more analyses of the clays. One fact seems certain, namely, that the clayey matrix of these so-called Glacial clays is not an original product, but a derivative one. That is to say, it was in the state of clay before it was mixed with the ingredients which it now contains, and was directly derived from various Secondary and Tertiary clays already formed: the Speeton and other clays of Yorkshire supplied the main part of the Yorkshire so-called Boulder-clays; the Kimeridge and Oxford Clays, together with the variegated Reading and other Eocene beds of the Fen country, supplying the clays further south. It was because these clays are exposed in Eastern England, and not elsewhere, that we have the

so-called Glacial beds so largely composed of clay there. The clayey matrix referred to was not only derivative, but it was derived from beds close by, and, so far as we know, was home-grown, and not imported from abroad.

As to the other ingredients of the so-called Glacial clays; these consist of two entirely different sets of materials—namely, the indigenous and home-grown on the one hand, and the foreigners or erratics on the other. It has been calculated by more than one observer that over 90 per cent. of the stones found in the Chalky Clay are of home growth; and it is probable that in all the clays of Eastern England generally referred to the so-called Glacial age, the proportion of home-grown boulders or stones is very largely indeed in excess of the strangers and foreigners.

The first and most important fact, therefore, to remember is that, over a very large area in Eastern England, the contents of the so-called Glacial clays, both the matrix and the stones in them, are of local origin and not imported. Most of the writers on these clays have been so impressed by the importance of these strangers which form barely a tithe of the contents of the clays, that they have neglected the more essential and more important lessons to be derived from an examination of the local and home-grown ingredients which they contain. I propose in this paper to entirely neglect the foreigners, and to converge attention upon the natives, reserving the discussion of the former for another occasion.

These natives consist of two entirely different classes. One class comprises more or less angular rubble, with its angles frequently blunted, and with the rude facets on its polygonal blocks often scratched; the other consisting of pebbles rolled perfectly smooth, and of various sizes, resembling sea- and river-shingle, these latter chiefly composed of flints and of quartzites, flints predominating greatly.

There can be no doubt whatever that these latter are the débris of marine and fluviatile shingles. It has apparently been argued by some that they were formed as pebbles during the so-called Glacial period. I believe this to be a complete mistake, and, so far as my own observations go—and I have worked pretty hard among them—they all seem to me to be derivative, and to be the débris of disintegrated Tertiary gravel and pebble beds. This view has been growing of late years.

Hutton, so far as I know, was the first to suggest that such gravels may have been the débris of the disintegration of older shingle beds. Thus he argues that these water-worn materials had their great roundness from the attrition caused by the waves of the sea upon some former coast, and that, after having been thus formed by *agitation on the shores, and transported into the deep*, this gravel contributed to the formation of Secondary strata, such as the pudding-stone he elsewhere described; and, lastly, that it was "from the decay and revolution of these Secondary strata, in the wasting operations of the surface, that have come those round siliceous bodies which could not be thus worn by travelling in the longest river." ("Theory of the Earth," vol. ii, p. 144, note.)

This view has been partially urged by Prestwich, and notably by Monckton, Herries, and the other younger geologists who have done so much to unravel the history of the Southern gravels. I would merely press their conclusions further, and affirm that all the rolled and rounded pebbles in the so-called Glacial clays and gravels are derivative, and have been derived from earlier Tertiary beds. I shall have more to say of them in a subsequent paper on the gravels and sands so frequently associated with the clays.

Turning from the polished pebbles to the angular and subangular rubble of home-made rocks, this consists of various kinds of Secondary strata, Lias, Oolite, and Chalk, distributed generally with a preponderance of Lias, Oolite, and Chalk fragments when the underlying beds correspond. These fragments, which have been partially shifted, and rolled and rubbed against each other, are, as I have tried to argue in two recent papers in the GEOLOGICAL MAGAZINE, the result entirely of the violent dislocation of the local Lias, Oolite, and Chalk solid beds, which has broken them up sometimes into great masses, and at others into mere road-metal. This disintegrated rubble was, in my view, ready made when the clay was compounded, and was in a large measure *in situ*; and the only alteration it underwent at the time when it was put into the portentous churn which turned out the chalky and other clays of Eastern England, was to have its angles blunted and its sides scratched and polished by being rolled and rubbed together. This seems as plain as plain can be if we are to explain the facts by inductive methods.

Whatever the forces or the machinery which mixed and distributed the so-called Boulder-clays of Eastern England, they had nothing to do with shaping the home-grown ingredients of the clays. These, with the exception of a little blunting of angles and polishing and some scratching, were already made, and are derivative. The process we have to analyze, therefore, is not complicated by questions as to the *modus operandi* by which the matrix of the clay itself and its contents were formed, but is limited to an explanation of how these ingredients were mixed and mingled together as we find them; and, secondly, how the product, when mixed, was distributed. What is most plain, *in limine*, is that, as the matrix of the clay and nine-tenths of its contents are local, it was fashioned on the spot, and was not imported. This follows from another fact. I have examined the coast of Norfolk and Suffolk with some care, and in those counties, as I have said, the Chalky Clay never reaches the sea, but occupies the projecting bluffs that form the highlands a few miles inland, while the country between them and the sea is occupied by the pebbly beds; the same is the case in Essex. This makes it clear that the Chalky Clay, which occupies the larger part of the interior of those counties, did not move westward from the seaboard, but came from the north or north-west, whence the chalk fragments and the clay itself were derived. This is again shown by the fact that this clay in Suffolk contains so many Liassic fossils. What is

true of the eastern boundary of the Chalky Clay is true also of its southern, western, and northern boundaries. The same conclusion follows from the complete absence of any shells or other marine débris from the Chalky Clay, showing it to have been an inland product of the denudation of local beds; and the evidence seems clear that whatever mixed and distributed the clay, the work itself was done on the spot.

Let us now examine the clay a little more closely.

The local stony rubble in the so-called Glacial clays of Eastern England is dispersed irregularly throughout the matrix. As has been frequently noted, the *prevailing* rock fragments in any district depends upon the substratum, but in every district known to me the rocks of other districts are represented. This means that whatever force or engine mixed the clays as we find them, it must have been one that could take up fragments of rock from the north and east and west, and move them in directions opposite and contrary to each other. The great cauldron in which the clay was mingled must have been occupied by some very powerful mobile machinery, which did not move in direct lines, but could move in various directions, and thus bring together and mix together the débris from the four points of the compass in one common medley, and having done so, could distribute it in the fashion we see it distributed now. What force or machinery was competent to effect this extraordinary work?

A considerable number of geologists unhesitatingly attribute the formation and its distribution to ice, and, in fact, point to this clay and instance it as one of the most remarkable proofs of ice-action in this country. I absolutely traverse this position: not only does it seem to me that the Chalky Clay was not distributed by ice, but I would go further and say that I cannot understand how ice in any shape can have formed and distributed it; and it is a very remarkable fact that those who have chiefly championed the cause of ice in this particular instance, are those who have never seen ice at work at all in Nature, while those who have so seen and studied it are unanimous, or almost unanimously of opinion, that nowhere in Nature is ice-work of the kind postulated to be found now going on anywhere.

The champions of ice have invoked it in several forms. Some of them invoke a foreign ice-sheet coming partly from Durham and partly from Scandinavia. This is supposed to be necessitated as a postulate by the presence in these clays of the foreign stones whose provenance has been deduced from Durham and Scandinavia respectively. We shall have more to say to this when we consider the foreign stones. At present we are dealing with the local ones only, and their distribution. Suppose that we can postulate such a foreign ice-sheet coming down, say, in Lincolnshire and the Fenland and their borders, what is the work which it must have been capable of doing when it reached England in order to explain the Chalky Clay. This vast, almost rigid, mass of ice pressed on from behind, it is presumed, by some tremendous *vis a tergo*, and

moving, if it moved like any ice known to us along definite lines of least resistance, must, when it reached England, have taken to sportively moving in all directions at once, not only towards its outward circumference sporadically, but also from its circumference inwards, in order to move the Oolitic and Lias fragments of Rutland, Northamptonshire, and Leicestershire, to the heights near Southwold, in Suffolk, the Red Chalk, carstone, and the limestones of Lincolnshire, far to the south of Cambridge, and the chalky fragments of the chalk exposures far to the west into middle England; and must have been capable of doing all these things at the same time, drawing in its scattered tribute to a common cauldron, and then distributing it, when mixed, as we see it distributed from Essex to Warwickshire. Have the fantastic attributes of man ever conceived a more preposterous mechanical process, and has science ever been burdened with such absurdities before?

Assuredly these facts make it impossible for those whose science is inductive to explain the mixing of the clays and their subsequent distribution by means of a foreign ice-sheet; and by foreign I mean here an ice-sheet, whencesoever derived, which has invaded Eastern England from the outside.

If we discard a foreign ice-sheet as the explanation of the Chalky Clay, shall we be any better off in making an appeal to a local glacier, or a series of local glaciers, as Searles Wood and others have done? In the first place, it must be remembered that such a local glacier does not in any way explain the crux of the position, namely, the presence of the foreign stones. Not only so, but if the area occupied by the Chalky Clay were occupied by local glaciers, how could it possibly be invaded by the foreigners at all? The presence of the local ice-sheet would form as complete a barrier to the introduction of foreign stones as a stone wall to the passage of the wind. This difficulty seems to have been entirely overlooked.

Let us pass on, however. The next difficulty is to understand how such *a local* ice-sheet or *local* glacier could be formed at all in the area in question when there are no mountains, and only low rolling downs. Glaciers, as we know them, gather on high ground, and descend into the valleys. If it were a local glacier which occupied the wolds of Lincolnshire, and thence distributed the products of its denuding agency, how comes it, again, that the débris of these chalk hills should be so different on the east side of them to what it is on the west? and how are we to account for the absence of chalk fragments in the beds to the east of these ridges? but apart from this, how are we to postulate glaciers as existing on lands so slightly elevated as are these wolds? Where is the gathering-ground for such glaciers to be found here? The same argument applies to the wolds of Norfolk or Yorkshire. If they were covered with local glaciers, how comes it that this ice shed such very different materials from their eastern and western flanks respectively? But suppose we got our local glaciers, each one crowning a different set of wolds, and moving outwards,

how can we possibly explain the collecting of stones from the east and the west, and the north, from areas entirely outside these local glaciers and their influence, and their distributing the mixed products far and wide right over the very areas supposed to have been occupied by the local glaciers?

Again, if the country were more or less blanketed by ice, either by a foreign ice-sheet or local glaciers, whence could the stones have been derived at all? There are no high mountains in Eastern England whose peaks would have projected above the ice, and been broken and weathered off to supply materials in this fashion. If the ice produced the disintegration, it must have been by digging up and excavating its own bed, and not in the fashion in which moraines are formed. Now this process of excavating a bed of rock underneath a moving mass of very heavy ice is not only mechanically incredible, but it is quite unsupported by any facts known to me, and is quite repudiated by Professor Bonney and others whose experience of ice is very much greater and more intimate than mine; and here we have to do, not with a local phenomenon occupying a few square yards or acres, but with a stupendous one involving the digging out and removing of wide stretches of rock, not in the form of mud or powder, but in some cases of great lumps and blocks of chalk and oolitic rock, over a wide area. Whether the ice postulated be foreign ice or native ice, it is equally impossible to understand how it could work in this fashion.

If we turn to the stones themselves, we shall have our view confirmed. True glacier-rubbed stones acquire a very curious contour, that is, so far as I know, never present in these chalk lumps of the Chalky Clay. In the case of all soft stones like chalk, they are rubbed down on two sides into flat cakes, so as to have two parallel faces, which are much scratched and furrowed, having, in fact, been used by the glacier as the "skid" of a coach is used. These are the real products of ice rubbing on soft stones, as known to myself in glacier districts, but such stones are markedly absent here. The stones in the Chalky Clay, etc., are as polygonal as those made by the stone-breakers on the highway, differing from such stones only in the fact that they range in size from dust up to masses many scores of feet long, and also in having their surface smoothed and their angles blunted. These are in no sense glacier stones. It is true that occasionally some are found which are scored and scratched, but those who appeal to these sporadic stones forget that whatever drove the clay along, if the movement were quick and two stones rubbed against each other, scratches or marks of rubbing must have ensued in such soft materials as chalk. Those who have examined the débris of such catastrophes as the Holmforth flood, etc., have noticed how invariably some of the stones are scored and scratched. It has been said that such stones are very scarce in shingles made by seas and rivers. Of course they are, because these shingles are formed of smoothed hard stones, like flint and chert and quartzite, which cannot scratch each other easily, but only rub each other down. Where a number of angular

stones, such as ballast, lie on a beach, and are examined after a gale, scratches and groovings can always be found. Professor Hughes, in an admirable paper published by the Cambridge Philosophical Society, has shown to how many adventitious causes such scratches can be traced; and "it is a long way to Loch Awe" when we invoke an ice-sheet or local icebergs to account for a few scratches on soft chalk stones, which are themselves otherwise absolutely different to true glacier stones, and have been clearly rolled and had their angles blunted by some other cause than ice. If the smaller lumps are difficult to explain by glacier action, *a fortiori*, as I have before argued, are the large masses, such as those in the contorted drift near Cromer, at Ely, in Rutlandshire, and the great masses of oolite in Lincolnshire, not only detached from their matrix but underlain by other so-called glacial deposits, and this, according to the land-ice hypothesis, all done under the tremendous pressure of such a heavy foot as its own gravid mass. It must be remembered also that among these very large transported rocks are, in some cases, great lumps of clay and of stratified sand, which have been moved *en masse*, and if moved under an ice-sheet must have been pounded and kneaded into a mere medley, and not had their lines of stratification intact. In these cases, at all events, the portage of the soft boulders must have been under the heavy foot of the ice, and not on its back.

Let us turn to another argument. In all glaciers known to me, the glacier products are distributed in a certain definite way, which at every point is different to that of the clays of Eastern England. Instead of being deposited in enormously thick masses near the focus of distribution, and gradually thinning out, glaciers deposit their greatest burdens at their furthest point in the form of mounds and ramparts and moraines. To a glacier it is indifferent whether a stone is big or small; big and little travel together, nor do we find the whole country, irrespective of its contour, as we do in this case, mantled and covered with continuous sheets of clay, which differs in texture and in aspect from all moraine matter known to me.

It was apparently the absence of anything like moraines which so impressed the late Professor Carvill Lewis, one of the most aggressive champions of glacial action, that he absolutely repudiated the presence of traces of ice in any form in East Anglia. If the ice here were an ice-sheet, we may well ask where is its great terminal moraine? and, if it were in the form of local glaciers, where are their lateral and terminal moraines?—where are the mounds and heaps of heterogeneous moraine stuff invariably present where glaciers have been at work?

Again, how are we to explain by ice the distribution of this clay in many places over perfectly horizontal layers of finely laminated sand and gravel, with the laminæ intact and undisturbed, as if the clay had been simply laid down on the sands by some gentle fingered agency? This is quite inconsistent with a moving mass of hard heavy ice, which would have kneaded and pounded the materials into a mass of what the American farmers call "muck."

Moreover, according to any rational mechanical theory, how can we account for the portage over wide areas of clay, scores of feet thick, under an ice-foot at all? How is it to seize it and move it *en masse* in the way required? It would surely slip over itself at once if the attempt were made to push it by means of a stupendously weighted and heavy ice-foot.

All these difficulties present themselves if we treat the glaciers as ordinary glaciers, such as we know them, moving down an ordinary set of hills, from the highlands to the lowlands. *A fortiori* do the difficulties become intensified when the particular contour of the country is considered. It must be remembered that the clayey matrix of the great mass of these clays only éxists in the lowest hollows, where the denudation has been greatest. It is thence that the Kimeridge and Oxford Clays must have come to form the mass of the Chalky Clay, and been thence distributed in various directions. It is in the low grounds of the Fen country where the churning and mixing of the materials must have been carried out, and it is thence the clay must afterwards have been sporadically spread out and scattered. How are we by any stretch of the imagination to realize an ice-sheet, formed in the deep hollow of the Fenlands, collecting together from the four winds of heaven materials for the clay, working and mixing them up in the deepest part of the area where it occurs, and then distributing it in various directions, *always* moving uphill from the trough on to the plateau? The kind of reasoning involved is assuredly going back to the dark ages of science, and getting away from induction altogether.

It does not seem possible to me that those who have postulated these local glaciers have ever really measured or thought out the conditions under which they would work at all. Mr. Jukes-Browne has stated some of the insurmountable difficulties in an excellent manner. Thus he says, writing of the Lincolnshire beds:—"The Boulder-clay is not disposed in the manner of moraines, but was clearly spread out as a universal mantle over the whole surface of the country. The ice which produced it certainly could not have been generated on the ridge itself, nor on any of the neighbouring hill ridges, and yet the materials of which the clay consists, and nearly all the stones it contains, are essentially local products derived from the rocks in the immediate neighbourhood. It is obvious that the chalk fragments must have been brought from the north-east, the Carboniferous rocks can only have come from the north or north-west, and the marlstone blocks travelled in all probability from west or south-west of the places where they are now found. . . . . When we consider the remarkable distribution of the stones and boulders in the clay of this area, the greater proportion of chalk detritus on the eastern slopes, and of Jurassic detritus on the western slopes, the fact that enormous masses of marlstone occur many miles to the eastward of the only place whence they can have been derived, the position of the large boulder of Cornbrash, near Ingoldsby, and the occurrence of Lower

Lias Limestone at Croxton, 300 feet above its level, the steep slopes of the Oolitic escarpment up which the ice must have passed, the difficulties in the way of applying the prevalent land-ice hypothesis become considerable." (Mem. S.W. Leicestershire, Sheet 70, pp. 82, 83.) I agree with every word of this, except the word 'considerable,' for which I should have substituted 'insurmountable.'

Land-ice in any form, therefore, seems quite incompetent to account for the Chalky Clay, nor can we invoke it without shutting our eyes to innumerable difficulties which at once arise. Some geologists have therefore had recourse to floating-ice, in the form of icebergs, or to coast-ice. This seems even a more desperate appeal. It first necessitates our conceding a general submergence of the country where the Boulder-clay is found, that is, as far as the Thames in the south and Warwickshire in the west. Granting this, how are we to solve by this agency any of the critical difficulties of the Chalky Clay; its existence in Lincolnshire to the west only of the Wolds; its existence everywhere in great insular areas separated from the sea; the complete absence of marine shells or débris from every place where it has been examined? How by such means as icebergs or shore-ice can we explain the covering of hundreds of miles of country with continuous blankets of chalky clay, not deposited in local heaps and masses but in sheets irrespective of the contour of the country, and in some cases deposited in very deep beds indeed, and most conspicuously on the higher grounds rather than the valleys? How could such agencies collect together oolitic blocks from Leicestershire and pieces of hard chalk from Norfolk, and mix them with the Oxford or Kimeridge Clays of the Fenland, or of the valley of the Ancholme, and then spread them out, as we find them spread out, from Suffolk in the east, into the Central Midlands?

Mr. Skertchly has stated the case against icebergs with singular force. "Icebergs," he says, "are the wrecks of land-ice, and the rocky material they carry is derived from the gathering-grounds of the parent ice; hence, if the Boulder-clay be iceberg drift, its components must be those of *the distant gathering-grounds*, and not those of the rocks it falls upon as the berg melts away." He goes on to say that as the chief ingredient in the clay is chalk, as it is found 300 feet above the present sea-level, not only must the gathering-ground have been on chalk but there must have been a submergence of at least 500 feet. This would convert the chalk area into a number of small islands, where it is not possible to understand glaciers gathering at all. Again, all the argillaceous matrix of the Chalky Clay is derived from the Fenland and the valley of the River Ancholme, and all the formations whence it could be derived would be under water. A more powerful argument remains in the fact that, if the clay be of iceberg origin, it can have no relation to the rocks on which it rests, except by accident; but the Chalky Clay does possess such a connection, and the icebergs must have had a selective affinity in shedding their burdens, by virtue of which they preferred to drop Kimeridge

Clay débris upon Kimeridge Clay and Oxford Clay detritus upon Oxford Clay. The clay is dark-blue on Kimeridge Clay, and light upon the light-blue Oxford Clay, besides which the fossils in the clay show a large percentage of Kimeridge Clay species where that rock lies below, and of Oxford Clay fossils where that is the subjacent bed. Not only so, but we find that the Kimeridge Chalky Clay invades the outcrop of the Oxford Chalky Clay, and the latter does not come on until the Oxford Clay has fairly taken the ground. In like manner the Boulder-clay is much more chalky on the Chalk than elsewhere, and this feature it maintains over the narrow Greensand outcrop, on to the Kimeridge Clay, as at Mareham, where chalky Boulder-clay is burned for lime. It is quite impossible, as Skertchly says, that icebergs should have dropped their burdens so geologically. ("Geology of the Fenland," pp. 215, 216.) The iceberg theory has, so far as I know, no friends left. I have already, in another paper, criticized the shore-ice theory, of which Mr. M. Reade has been the champion. He has invoked it to explain the portage of the vast blocks which so often occur in the Chalky Clay; but he would hardly attribute to coast-ice the collecting of the materials of the Chalky Clay, the mingling of them into the present medley, and the distribution of the mixture far and wide over one-third of England.

Whichever way the problem is approached, the intervention of ice as a *causa causans* seems impossible. It was appealed to in reality to explain phenomena whose explanation is to be sought for in very different causes—(1) the scratching of certain chalk masses, a result which must have followed from any theory of the distribution of the Chalk involving its portage from one place to another, which we all concede; (2) the presence in the eastern drifts of vast continuous masses of chalk, etc., which, as we have seen, is due to an entirely different cause; and, lastly, the presence of the foreign boulders. This last fact I hope to discuss on another occasion, when I should like to correlate with the clays we have been discussing the gravels and so-called Middle Sands of Eastern England, in which these foreign stones also occur. In the meantime the view I would press is, that these clays present no single feature consistent with their having been deposited by ice, nor do I know of any reason which is sound for invoking ice to explain them. The view is held by others besides myself, on other grounds. Thus, Mr. (now Professor) Seeley says:—"I have not found in this locality a vestige of iceberg action. Of glacier action the deposits of the Fenlands offer no traces, unless the fragments of northern rocks be held to prove that one great glacier stretched from the Tweed to the Thames, of which there may be as much likelihood as that the ice of the Caucasus excavated the Black Sea." (Geol. Mag., 1866, Vol. III. p. 496.) Mr. Jukes-Browne's opinion I have already quoted, but the most unexpected and trenchant view of all is that of the late Professor Carvill Lewis, who was one of the archpriests of Ultra-Glacialism, and who in regard to East Anglia emphatically took the same side.

REVIEWS.

CRYSTALLOGRAPHY FOR BEGINNERS, WITH AN APPENDIX ON THE USE OF THE BLOWPIPE AND THE DETERMINATION OF COMMON MINERALS (after the Method of Dr. ALBIN WEISBACH). By C. J. WOODWARD, B.Sc. Pp. 164, 4 Plates, and 75 Woodcut Illustrations. (London: Simpkin, Marshall, Hamilton, Kent and Co., 1896.)

THIS work consists of two parts, really independent of each other: the one treats of Crystallography (100 pages); the other (termed merely an Appendix, though extending to more than half the length of the first) consists almost entirely of tables relating to the determination of the more common minerals. The author has avoided technicalities of expression as far as is possible, and has aimed throughout at extreme simplicity of literary style, as will be clear from one of the opening sentences of the First Lesson: "Put into a pipkin, or an earthenware basin, about half a pint of water, and add two to three ounces of the blue vitriol, and heat to near boiling." In spite of the difficulty inseparable from a subject which involves the geometry of solid bodies, the meaning of the author is always evident to the thoughtful reader. In a brief introduction Mr. Woodward first explains the method of preparing pasteboard models for the illustration of the lessons, and supplies two plates of "nets" to be used for this purpose. He then treats successively of the constancy of the angles of crystals, the idea of symmetry, the systems of crystallization, the notation of forms, the drawing of crystals, and the stereographic projection of poles. The discussion of the physical characters of crystals extends over only 15 pages, and is followed by a brief account of mero-symmetry. At the end of each of the nine lessons is given a series of questions for the mental exercise of the student, and the whole is illustrated with numerous diagrams. Although some inaccuracies of statement are obvious to the critical eye, they are not of a character to interfere with the general usefulness of the book. The Appendix, as is stated by the author, is virtually an extract from the more complete tables published by Professor Weisbach, of Freiberg, and will be of service to the mineralogical beginner. Sets of six illustrative specimens and some pieces of simple apparatus have been prepared for issue to students using the book.

REPORTS AND PROCEEDINGS.

BRITISH ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE. ADDRESS TO THE GEOLOGICAL SECTION by Professor J. E. MARR, M.A., F.R.S., Sec.G.S., President of the Section, Liverpool, September 17, 1896.

THE feelings of one who, being but little versed in the economic applications of his science, is called upon to address a meeting of the Association held in a large industrial centre, might, under ordinary circumstances, be of no very pleasant character; but I take courage when I remember that those connected with my native county, in which we

are now gathered, have taken prominent part in advancing branches of our science which are not directly concerned with industrial affairs. I am reminded, for instance, that one amongst you, himself a busy professional man, has in his book on "The Origin of Mountain Ranges" given to the world a theoretical work of the highest value; that, on the opposite side of the county, those who are responsible for the formation and management of that excellent educational institution, the Ancoats Museum, have wisely recognized the value of some knowledge of geology as a means of quickening our appreciation of the beauties of Nature; and that one who has done solid service to geology by his teachings, who has kept before us the relationship of our science to that which is beautiful—I refer to the distinguished author of "Modern Painters"—has chosen the northern part of the county for his home, and has illustrated his teaching afresh by reference to the rocks of the lovely district around him. Nor can I help referring to one who has recently passed away—the late Sir Joseph Prestwich—the last link between the pioneers of our science and the geologists of the present day, who, though born in London, was of Lancashire family, and whom we may surely therefore claim as one of Lancashire's worthies. With these evidences of the catholicity of taste on the part of geologists connected with the county, I feel free to choose my own subject for this address, and, my time being occupied to a large extent with academic work, I may be pardoned for treating that subject in academic fashion. As I have paid considerable attention to the branch of the science which bears the somewhat uncouth designation of stratigraphical geology, I propose to take the present state of our knowledge of this branch as my theme.

Of the four great divisions of geology, petrology may be claimed as being largely of German origin, the great impetus to its study having been given by Werner and his teachings. Palæontology may be as justly claimed by the French nation, Cuvier having been to so great an extent responsible for placing it upon a scientific basis. Physical geology we may partly regard as our own, the principles laid down by Hutton and supported by Playfair having received illustration from a host of British writers, amongst whom may be mentioned Jukes, Ramsay, and the brothers Geikie; but the grand principles of physical geology have been so largely illustrated by the magnificent and simple features displayed on the other side of the Atlantic, that we may well refer to our American brethren as leaders in this branch of study. The fourth branch, stratigraphical geology, is essentially British as regards origin, and, as everyone is aware, its scientific principles were established by William Smith, who was not only the father of English geology, but of stratigraphical geology in general.

Few will deny that stratigraphical geology is the highest branch of the science, for, as has been well said, it "gathers up the sum of all that is made known by the other departments of the science, and makes it subservient to the interpretation of the geological history of the earth." The object of the stratigraphical geologist is to obtain information concerning all physical, climatic, and biological events which have occurred during each period of the past, and to arrange them in chronological order, so as to write a connected history of the earth. If all of this information were at our disposal, we could write a complete earth-history, and the task of the geologist would be ended. As it is, we have barely crossed the threshold of discovery, and the "imperfection of the geological record," like the "glorious uncertainty" of our national game, gives geology one of its great charms. Before passing on to consider more particularly the present state of the subject of our study, a few remarks upon this imperfection of the geological record may not be out of place,

seeing that the term has been used by so many modern writers, and its exact signification occasionally misunderstood. The imperfection of the palæontological record is usually understood by the term when used, and it will be considered here as an illustration of the incompleteness of our knowledge of earth-history; but it must be remembered that the imperfection of the physical record is equally striking, as will be insisted on more fully in the sequel.

Specially prominent amongst the points upon which we are ignorant stands the nature of the pre-Cambrian faunas. The extraordinary complexity of the earliest known Cambrian fauna has long been a matter for surprise, and the recent discoveries in connection with the *Olenellus* fauna do not diminish the feeling.[1] After commenting upon the varied nature of the earliest known fauna, the late Professor Huxley, in his Address to the Geological Society in 1862, stated that "any admissible hypothesis of progressive modification must be compatible with persistence without progression, through indefinite periods. . . . . Should such an hypothesis eventually be proved to be true, ... the conclusion will inevitably present itself, that the Palæozoic, Mesozoic, and Cainozoic faunæ and floræ, taken together, bear somewhat the same proportion to the whole series of living beings which have occupied this globe, as the existing fauna and flora do to them." Whether or not this estimate is correct, all geologists will agree that a vast period of time must have elapsed before the Cambrian period, and yet our ignorance of faunas existing prior to the time when the *Olenellus* fauna occupied the Cambrian seas is almost complete. True, many pre-Cambrian fossils have been described at various times, but, in the opinion of many competent judges, the organic nature of each one of these requires confirmation. I need not, however, enlarge upon this matter, for I am glad to say we have amongst us a geologist who will at a later stage read a paper before this Section upon the subject of pre-Cambrian fossils, and there is no one better able, owing to his intimate acquaintance with the actual relics, to present fairly and impartially the arguments which have been advanced in favour of the organic origin of the objects which have been appealed to as evidences of organisms of pre-Cambrian age than our revered co-worker from Canada, Sir J. W. Dawson. We may look forward with confidence to the future discovery of many faunas older than those of which we now possess certain knowledge, but until these are discovered the palæontological record must be admitted to be in a remarkably incomplete condition. In the meantime, a study of the recent advance of our knowledge of early life is significant of the mode in which still earlier faunas will probably be brought to light In 1845 Dr. E. Emmons described a fossil, now known to be an *Olenellus*, though at that time the earliest fauna was supposed to be one containing a much later group of organisms, and it was not until Nathorst and Brögger established the position of the *Olenellus* zone that the existence of a fauna earlier than that of which *Paradoxides* was a member was admitted; and, indeed, the *Paradoxides* fauna itself was proved to be earlier than that containing *Olenus*, long after these two genera had been made familiar to palæontologists, the Swedish palæontologist, Angelin, having referred the *Paradoxides* fauna to a period earlier than that of the one with *Olenus*. It is quite possible, therefore, that fossils are actually preserved in our museums at the present moment which have been extracted from rocks deposited before the period of

[1] Dr. C. D. Walcott, in his monograph on "The Fauna of the Lower Cambrian or *Olenellus* Zone" (Washington, 1890), records the following great groups as represented in the *Olenellus* beds of America: Spongiæ, Hydrozoa, Actinozoa, Echinodermata, Annelida? (trails, burrows, and tracks), Brachiopoda, Lamellibranchiata, Gasteropoda, Pteropoda, Crustacea, and Trilobita. Others are known as occurring in beds of the same age in the Old World.

formation of the *Olenellus* beds, though their age has not been determined. The *Olenellus* horizon now furnishes us with a datum-line from which we can work backwards, and it is quite possible that the *Neobolus* beds of the Salt Range,[1] which underlie beds holding *Olenellus*, really do contain, as has been maintained, a fauna of date anterior to the formation of the *Olenellus* beds; and the same may be the case with the beds containing the *Protolenus* fauna in Canada,[2] for this fauna is very different from any known in the *Olenellus* beds, or at a higher horizon, though Mr. G. F. Matthew, to whom geologists owe a great debt for his admirable descriptions of the early fossils of the Canadian rocks speaks very cautiously of the age of the beds containing *Protolenus* and its associates. Notwithstanding our ignorance of pre-Cambrian faunas, valuable work has recently been done in proving the existence of important groups of stratified rocks deposited previously to the formation of the beds containing the earliest known Cambrian fossils; I may refer especially to the proofs of the pre-Cambrian age of the Torridon Sandstone of North-west Scotland lately furnished by the officers of the Geological Survey, and their discovery that the maximum thickness of these strata is over 10,000 feet.[3] Amongst the sediments of this important system, more than one fauna may be discovered, even if most of the strata were accumulated with rapidity, and all geologists must hope that the officers of the Survey—who, following Nicol, Lapworth, and others, have done so much to elucidate the geological structure of the Scottish Highlands—may obtain the legitimate reward of their labours, and definitely prove the occurrence of rich faunas of pre-Cambrian age in the rocks of that region.

But although we may look forward hopefully to the time when we may lessen the imperfection of the records of early life upon the globe, even the most hopeful cannot expect that record to be rendered perfect, or that it will make any near approach to perfection. The posterior segments of the remarkable trilobite *Mesonacis Vermontana* are of a much more delicate character than the anterior ones, and the resemblance of the spine on the fifteenth "body-segment" of this species to the terminal spine of *Olenellus* proper, suggests that in the latter subgenus posterior segments of a purely membranous character may have existed, devoid of hard parts. If this be so, the entire outer covering of the trilobites, at a period not very remote from the end of pre-Cambrian times, may have been membranous, and the same thing may have occurred with the structures analogous to the hard parts of organisms of other groups. Indeed, with our present views as to development, we can scarcely suppose that organisms acquired hard parts at a very early period of their existence, and fauna after fauna may have occupied the globe, and disappeared, leaving no trace of its existence, in which case we are not likely ever to obtain definite knowledge of the characters of our earliest faunas, and the biologist must not look to the geologist for direct information concerning the dawn of life upon the earth.

Proceeding now to a consideration of the faunas of the rocks formed after pre-Cambrian times, a rough test of the imperfection of the record may be made by examining the gaps which occur in the vertical distribution of forms of life. If our knowledge of ancient faunas were very incomplete, we ought to meet with many cases of recurrence of forms after their apparent disappearance from intervening strata of considerable thickness, and many such cases have actually been described by that eminent

[1] See F. Noetling, "On the Cambrian Formation of the Eastern Salt Range": Records Geol. Survey India, vol. xxvii, p. 71.

[2] G. F. Matthew, "The *Protolenus* Fauna": Trans. New York Acad. of Science, 1895, vol. xiv, p. 101.

[3] Sir A. Geikie, "Annual Report of the Geological Survey [United Kingdom] . . . for the year ending December 31, 1893." London, 1894.

palæontologist, M. Barrande, amongst the Palæozoic rocks of Bohemia, though even these are gradually being reduced in number owing to recent discoveries; indeed, in the case of the marine faunas, marked cases of recurrence are comparatively rare, and the occurrence of each form is generally fairly unbroken from its first appearance to its final extinction, thus showing that the imperfection of the record is by no means so marked as might be supposed. Fresh-water and terrestrial forms naturally furnish a large percentage of cases of recurrence, owing to the comparative rarity with which deposits containing such organisms are preserved amongst the strata.

A brief consideration of the main reasons for the present imperfection of our knowledge of the faunas of rocks formed subsequently to pre-Cambrian times may be useful, and suggestive of lines along which future work may be carried out. That detailed work in tracts of country which are yet unexplored, or have been but imperfectly examined by the geologist, will add largely to our stock of information, needs only to be mentioned; the probable importance of work of this kind in the future may be inferred from a consideration of the great increase of our knowledge of the Permo-Carboniferous faunas, as the result of recent labours in remote regions. It is specially desirable that the ancient faunas and floras of tropical regions should be more fully made known, as a study of these will probably throw considerable light upon the influence of climate upon the geographical distribution of organisms in past times. The old floras and faunas of Arctic regions are becoming fairly well known, thanks to the zeal with which the Arctic regions have been explored. But, confining our attention to the geology of our own country, much remains to be done even here, and local observers especially have opportunities of adding largely to our stock of knowledge, a task they have performed so well in the past. To give examples of the value of such work, our knowledge of the fauna of the Cambrian rocks of Britain is largely due to the present President of the Geological Society, when resident at St. David's, whilst the magnificent fauna of the Wenlock Limestone would have been far less perfectly known than it is if it were not for the collections of men like the late Colonel Fletcher and the late Dr. Grindrod. Again, the existence of the rich fauna of the Cambridge Greensand would have been unsuspected, had not the bed known by that name been worked for the phosphatic nodules which it contains.

It is very desirable that large collections of varieties of species should be made, for in this matter the record is very imperfect. There has been, and, I fear, is still, a tendency to reject specimens when their characters do not conform with those given in specific descriptions, and thus much valuable material is lost. Local observers should be specially careful to search for varieties, which may be very abundant in places where the conditions were favourable for their production, though rare or unknown elsewhere. Thus, I find the late Mr. W. Keeping remarking that "it is noteworthy that at Upware, and, indeed, all other places known to me, the species of Brachiopoda [of the Neocomian beds] maintain much more distinctness and isolation from one another than at Brickhill."[1] The latter place appears to be one where conditions were exceptionally favourable in Neocomian times for the production of intermediate forms.

A mere knowledge of varieties is, however, of no great use to the collector without a general acquaintance with the morphology of the organisms whose remains he extracts from the earth's strata, and one who has this can do signal service to the science. It is specially important that local observers should be willing to devote themselves to the study of particular groups of organisms, and to collect large suites of specimens

[1] W. Keeping, Sedgwick Essay: "The Fossils and Palæontological Affinities of the Neocomian Deposits of Upware and Brickhill." Cambridge, 1883.

of the group they have chosen for study. With a group like the graptolites, for instance, the specimens which are apparently best preserved are often of little value from a morphological point of view, and fragments frequently furnish more information than more complete specimens. These fragments seldom find their way to our museums, and accordingly we may examine a large suite of graptolites in those museums without finding any examples showing particular structures of importance, such as the sac-like bodies carried by many of these creatures. As an illustration of the value of work done by one who has made a special study of a particular group of organisms, I may refer to the remarkable success achieved by the late Mr. Norman Glass in developing the calcareous supports of the brachial processes of Brachiopods. Work of this character will greatly reduce the imperfection of the record from the biologists' point of view.

The importance of detailed work leads one to comment upon the general methods of research which have been largely adopted in the case of the stratified rocks. The principle that strata are identifiable by their included organisms is the basis of modern work, as it was of that which was achieved by the father of English Geology, and the identification of strata in this manner has of recent years been carried out in very great detail, notwithstanding the attempt on the part of some well-known writers to show that correlation of strata in great detail is impossible. The objection to this detailed work is mainly founded upon the fact that it must take time for an organism or group of organisms to migrate from one area to another, and therefore it was stated that they cannot have lived contemporaneously in two remote areas. But the force of this objection is practically done away with if it can be shown that the time taken for migration is exceedingly short as compared with the time of duration of an organism or group of organisms upon the earth, and this has been shown in the only possible way—namely, by accumulating a very great amount of evidence as the result of observation. The eminent writers referred to above, who were not trained geologists, never properly grasped the vast periods of time which must have elapsed during the occurrence of the events which it is the geologist's province to study. An historian would speak of events which began at noon on a certain day and ended at midnight at the close of that day as contemporaneous with events which commenced and ended five minutes later, and this is quite on a par with what the geologist does when correlating strata. Nevertheless, there are many people who still view the task of correlating minute subdivisions of stratified systems with one another, with a certain amount of suspicion, if not with positive antipathy; but the work must be done for all that. Brilliant generalizations are attractive as well as valuable, but the steady accumulation of facts is as necessary for the advancement of the science as it was in the days when the Geological Society was founded, and its members applied themselves "to multiply and record observations, and patiently to await the result at some future period." I have already suggested a resemblance between geology and cricket, and I may be permitted to point out that just as in the game the free-hitter wins the applause, though the patient "stone-waller" often wins the match, so, in the science, the man apt at brilliant generalizations gains the approval of the general public, but the patient recorder of apparently insignificant details adds matter of permanent value to the stores of our knowledge. In the case of stratigraphical geology, if we were compelled to be content with correlation of systems only, and were unable to ascertain which of the smaller series and stages were contemporaneous, but could only speak of these as "homotaxial," we should be in much the same position as the would-be antiquary who was content to consider objects fashioned by the Romans as contemporaneous with those of mediæval

times. Under such circumstances geology would indeed be an uncertain science, and we should labour in the field knowing that a satisfactory earth-history would never be written. Let us hope that a brighter future is in store for us, and let me urge my countrymen to continue to study the minute subdivisions of the strata, lest they be left behind by the geologists of other countries, to whom the necessity for this kind of study is apparent, and who are carrying it on with great success.

The value of detailed work on the part of the stratigraphical geologist is best grasped if we consider the recent advance that has been made in our science owing to the more or less exhaustive survey of the strata of various areas, and the application of the results obtained to the elucidation of the earth's history. A review of this nature will enable us not only to see what has been done, but also to detect lines of inquiry which it will be useful to pursue in the future; but it is obvious that the subject is so wide that little more can be attempted than to touch lightly upon some of the more prominent questions. A work might well be written treating of the matters which I propose to notice. We have all read our "Principles of Geology," or "The Modern Changes of the Earth and its inhabitants considered as illustrative of Geology," to quote the alternative title; some day we may have a book written about the ancient changes of the earth and its inhabitants considered as illustrative of geography.

Commencing with a glance at the light thrown on inorganic changes by a detailed examination of the strata, I may briefly allude to advances which have recently been made in the study of denudation. The minor faults, which can only be detected when the small subdivisions of rock-groups are followed out carefully on the ground, have been shown to be of great importance in defining the direction in which the agents of denudation have operated, as demonstrated by Professor W. C. Brögger, for instance, in the case of the Christiania Fjord[1]; and I have recently endeavoured to prove that certain valleys in the English Lake District have been determined by shattered belts of country, the existence of which is shown by following thin bands of strata along their outcrop. The importance of the study of the strata in connection with the genesis and subsequent changes of river-systems is admirably brought out in Professor W. M. Davis's paper on "The Development of certain English Rivers,"[2] a paper which should be read by all physical geologists; it is, indeed, a starting-point for kindred work which remains especially for local observers to accomplish. Study of this kind not only adds to our knowledge of the work of geological agencies, but helps to diminish the imperfection of the record, for the nature of river-systems, when rightly understood, enables us to detect the former presence of deposits over areas from which they have long since been removed by denudation.

An intimate acquaintance with the lithological characters of the strata of a district affords valuable information in connection with the subject of glacial denudation. The direction of glacial transport over the British Isles has been largely inferred from a study of the distribution of boulders of igneous rock, whilst those of sedimentary rock have been less carefully observed. The importance of the latter is well shown by the work which has been done in Northern Europe in tracing the Scandinavian boulders to their sources, a task which could not have been performed successfully if the Scandinavian strata had not been studied in great detail.[3] I shall

[1] W. C. Brögger, Nyt. Mag. for Naturvidensk., vol. xxx (1886), p. 79.

[2] W. M. Davis, Geograph. Journ., vol. v (1895), p. 127.

[3] It is desirable that the boulders of sedimentary rock imbedded in the drifts of East Anglia should be carefully examined, and fossils collected from them. The calcareous strata associated with the Alum Shales of Scandinavia and the strata of the *Orthoceras* Limestone of that region may be expected to be represented amongst the boulders.

presently have more to say with regard to work connected with the lithological characters of the sediments. Whilst mentioning glacial denudation, let me allude to a piece of work which should be done in great detail, though it is not, strictly speaking, connected with stratigraphy, namely, the mapping of the rocks around asserted "rock-basins." I can find no actual proof of the occurrence of such basins in Britain, and it is very desirable that the solid rocks and the drift should be carefully inserted on large-scale maps, not only all around the shores of several lakes, but also between the lakes and the sea, in order to ascertain whether the lakes are really held in rock-basins. Until this work is done, however probable the occurrence of rock-basins in Britain may be considered to be, their actual existence cannot be accepted as proved.

When referring to the subject of denudation, mention was made a moment ago of the study of the lithological character of the sediments. Admirable work in this direction was carried out years ago by one who may be said to have largely changed the direction of advance of geology in this country owing to his researches "On the Microscopical Structure of Crystals, indicating the Origin of Minerals and Rocks." I refer, of course, to Dr. H. C. Sorby. But since our attention has been so largely directed to petrology, the study of the igneous and metamorphic rocks has been most zealously pursued, whilst that of the sediments has been singularly little heeded, with few exceptions, prominent amongst which is the work of Mr. Maynard Hutchings, the results of which have been recently published in the GEOLOGICAL MAGAZINE, though we must all hope that the details which have hitherto been supplied to us, valuable as they are, are only a foretaste of what is to follow from the pen of this able observer. Descriptions of the lithological changes which occur in a vertical series of sediments, as well as of those which are observed when any particular band is traced laterally, will no doubt throw light upon a number of interesting questions.

Careful work amongst the ancient sediments, especially those which are of organic origin, has strikingly illustrated the general identity of characters, and therefore of methods of formation, of deposits laid down on the sea-floors of past times and those which are at present in course of construction. Globigerine-oozes have been detected at various horizons and in many countries. Professor H. Alleyne Nicholson [1] has described a pteropod-ooze of Devonian age in the Hamilton Limestone of Canada, which is largely composed of the tests of *Styliola* ; and to Dr. G. J. Hinde we owe the discovery of a large number of radiolarian cherts of Palæozoic and Neozoic ages in various parts of the globe. The extreme thinness of many argillaceous deposits, which are represented elsewhere by hundreds of feet of strata, suggests that some of them, at any rate, may be analogous to the deep-sea clays of modern oceans, though in the case of deposits of this nature we must depend to a large extent upon negative evidence. The uniformity of character of thin marine deposits over wide areas is in itself evidence of their formation at some distance from the land ; but although the proofs of origin of ancient sediments far from coast-lines may be looked upon as permanently established, the evidence for their deposition at great depths below the ocean's surface might be advantageously increased in the case of many of them. The fairly modern sediments, containing genera which are still in existence, are more likely to furnish satisfactory proofs of a deep-sea origin than are more ancient deposits. Thus the existence of *Archæopneustes* and *Cystechinus* in the oceanic series of Barbadoes, as described by Dr. J. W. Gregory, furnishes strong proofs of the deep-sea character of the deposits ; whilst the only actual argument in favour of the deep-sea character of certain Palæozoic sediments has been put forward by

[1] Nicholson and Lydekker, "Manual of Palæontology," chap. ii.

Professor Suess, who notes the similarity of certain structures of creatures in ancient rocks to those possessed by modern deep-sea crustacea, especially the co-existence of trilobites which are blind with those which have enormously developed eyes.

A question which has been very prominently brought to the fore in recent years is that of the mode of formation of certain coral-reefs. The theory of Charles Darwin, lately so widely accepted as an explanation of the mode of formation of barrier-reefs and atolls, has been, as is well known, criticized by Dr. Murray, with the result that a large number of valuable observations have been recently made on modern reefs, especially by biologists, as a contribution to the study of reef formation. Nor have geologists been inactive. Dr. E. Mojsisovics and Professor Dupont, to mention two prominent observers, have described knoll-like masses of limestone more or less analogous, as regards structure, to modern coral-reefs. They consider that these have been formed by corals, and, indeed, Dupont maintains that the atoll-shape is still recognizable in ancient Devonian coral-reefs in Belgium.[1] I would observe that all cases of "knoll-reefs" of this character have been described in districts which furnish proofs of having been subjected to considerable orogenic disturbance, subsequent to the formation of the rocks composing the knoll-shaped masses, whilst in areas which have not been affected by violent earth foldings, the reef-building corals, so far as I have been able to ascertain, give rise to sheet-like masses, such as should be produced according to Dr. Murray's theory. I would mention especially the reefs of the Corallian rocks of England, and also some admirable examples seen amongst the Carboniferous Limestone strata of the great western escarpment of the Pennine Chain which faces the Eden Valley in the neighbourhood of Melmerby in Cumberland. Considering the number of dissected coral-reefs which exist amongst the strata of the earth's crust, and the striking way in which their structure is often displayed, it is rather remarkable that comparatively little attention has been paid to them by geologists in general, when the subject has been so prominently brought before the scientific world, for we must surely admit that we are much more likely to gain important information, shedding light upon the methods of reef-formation, by a study of such dissected reefs, than by making a few boreholes on some special coral island. I would specially recommend geologists to make a detailed study of the British coral-reefs of Silurian, Devonian, Carboniferous, and Jurassic ages.

Turning now to organic deposits of vegetable origin, we must, as a result of detailed work, be prepared to admit the inapplicability of any one theory of the formation of coal-seams. The "growth-in-place" theory may be considered fairly well established for some coals, such as the spore-coals, whilst the "drift" theory furnishes an equally satisfactory explanation of the formation of cannel-coal. It is now clear that the application of the general term *coal* to a number of materials of diverse nature, and probably of diverse origin, was largely responsible for the dragging-out of a controversy in which the champions of either side endeavoured to explain the origin of all coal in one particular way.

The stratigraphical geologist, attempting to restore the physical geography of former periods, naturally pays much attention to the positions of ancient coast-lines; indeed, all teachers find it impossible to give an intelligible account of the stratified rocks without some reference to the distribution of land and sea at the time of their formation. The general position of land-masses at various times has been ascertained in

[1] Similar knoll-like masses have been described in this country by Mr. R. H. Tiddeman, as occurring in the Craven district of Yorkshire, but he does not attribute their formation to coral-growth to any great extent.

several parts of the world, but much more information must be gathered together before our restorations of ancient sea-margins approximate to the truth. The Carboniferous rocks of Britain have been specially studied with reference to the distribution of land and water during the period of their accumulation, and yet we find that owing to the erroneous identification of certain rocks of Devonshire as grits or sandstones, which Dr. Hinde has shown to be radiolarian cherts, land was supposed to lie at no great distance south of this region in Lower Carboniferous times, whereas the probabilities are in favour of the existence of an open ocean at a considerable distance from any land in that direction. This case furnishes us with an excellent warning against generalization upon insufficient data.

As a result of detailed study of the strata, the effects of earth-movements have been largely made known to us, especially of those comparatively local disturbances spoken of as orogenic which are mainly connected with mountain-building, whilst information concerning the more widely-spread epeirogenic movements is also furnished by a study of the stratified rocks. The structure of the Alps, of the North-West Highlands of Scotland, and of the uplifted tracks of North America is now familiar to geologists, whilst the study of comparatively recent sediments has proved the existence of widespread and extensive movements in times which are geologically modern; for instance, the deep-water deposits of late Tertiary age found in the West Indies indicate the occurrence of considerable uplift in that region. But a great amount of work yet remains to be done in this connection, especially concerning horizontal distortion of masses of the earth's crust, owing to more rapid horizontal advance of one portion than of another, during periods of movement. Not until we gather together a large amount of information derived from actual inspection of the rocks shall we be able to frame satisfactory theories of earth-movement, and in the meantime we are largely dependent upon the speculations of the physicist, often founded upon very imperfect data, on which is built an imposing superstructure of mathematical reasoning. We have been told that our continents and ocean-basins have been to a great extent permanent as regards position through long geological ages; we now reply by pointing to deep-sea sediments of nearly all geological periods, which have been uplifted from the ocean-abysses to form portions of our continents; and as the result of study of the distribution of fossil organisms, we can point almost as confidently to the sites of old continents now sunk down into the ocean depths. It seems clear that our knowledge of the causes of earth-movements is still in its infancy, and that we must be content to wait awhile, until we have further information at our disposal.

Recent work has proved the intimate connection betwixt earth-movement and the emission and intrusion of igneous rocks, and the study of igneous rocks has advanced beyond the petrographical stage; the rocks are now made to contribute their share towards the history of different geological periods. The part which volcanic action has played in the actual formation of the earth's crust is well exemplified in Sir Archibald Geikie's Presidential Addresses to the Geological Society, wherein he treats of the former volcanic history of the British Isles.[1] The way in which extruded material contributes to the formation of sedimentary masses has, perhaps, not been fully grasped by many writers, who frequently seem to assume that deposition is a measure of denudation, and *vice versâ*, whereas deposition is only a measure of denudation, and of the material which has been ejected in a fragmental condition from the earth's interior, which in some places forms a very considerable percentage of the total amount of sediment.

[1] Sir A. Geikie, Quart. Journ. Geol. Soc., vols. xlvii and xlviii.

The intruded rocks also throw much light on past earth-history, and I cannot give a better illustration of the valuable information which they may furnish to the stratigraphical geologist, when rightly studied, than by referring to the excellent and suggestive work by my colleague, Mr. Alfred Harker, on the Bala Volcanic Rocks of Carnarvonshire.[1]

Perhaps the most striking instance of the effect which detailed stratigraphical work has produced on geological thought is supplied by the study of the crystalline schists. Our knowledge of the great bulk of the rocks which enter into the formation of a schistose complex is not very great, but the mode of production of many of them is now well known, and the crude speculations of some of the early geologists are now making way for theories founded on careful and minute observations in the field as well as in the laboratory. Recent work amongst the crystalline schists shows, furthermore, how careful we should be not to assume that because we have got at the truth we have therefore ascertained the whole truth. We all remember how potent a factor dynamic metamorphism was supposed to be, owing to discoveries made in the greatly disturbed rocks of Scotland and Switzerland ; and the action of heat was almost ignored by some writers, except as a minor factor, in the production of metamorphic change. The latest studies amongst the foliated rocks tend to show that heat does play a most important part in the manufacture of schists. The detailed work of Mr. George Barrow, in North-east Forfarshire,[2] has already thrown a flood of light upon the origin of certain schists, and their connection with igneous rocks, and geologists will look forward with eagerness to further studies of the puzzling Highland rocks by this keen observer.

The subject of former climatic conditions is one in which the geologist has very largely depended upon followers of other branches of science for light, and yet it is one peculiarly within the domain of the stratigraphical geologist ; and information which has already been furnished concerning former climatic conditions, as the result of careful study of the strata, is probably only an earnest of what is to follow when the specialist in climatology pays attention to the records of the rocks, and avoids the theories elaborated in the student's sanctum. The recognition of an Ice Age in Pleistocene times at once proved the fallacy of the supposition that there has been a gradual fall in temperature throughout geological ages without any subsequent rise, and accordingly most theories which have been put forward to account for former climatic change have been advanced with special reference to the Glacial period or periods, although there are many other interesting matters connected with climate with which the geologist has to deal. Nevertheless, the occurrence of Glacial periods is a matter of very great interest, and one which has deservedly received much attention, though the extremely plausible hypothesis of Croll, and the clear manner in which it has been presented to general readers, tended to throw other views into the shade, until quite recently, when this hypothesis has been controverted from the point of view of the physicist. In the meantime considerable advance has been made in our actual knowledge, and this year, probably for the first time, and as the result of the masterly *résumé* of Professor Edgworth David,[3] the bulk of British geologists is prepared to admit that there has been more than one Glacial period, and that the evidence of glacial conditions in the southern hemisphere in Permo-Carboniferous times is established. Croll's hypothesis of course requires the recurrence of Glacial periods, but leaving out of account arguments not of a geological character, which have been advanced against

[1] Alfred Harker, Sedgwick Essay for 1888 (Camb. Univ. Press, 1889).

[2] G. Barrow, Quart. Journ. Geol. Soc., vol. xlix (1893), p. 330.

[3] T. W. E. David, "Evidences of Glacial Action in Australia in Permo-Carboniferous Time": Quart. Journ. Geol. Soc., vol. lii, p. 289.

this hypothesis, the objection raised by Messrs. Gray and Kendall,[1] that in the case of the Pleistocene Ice Age "the cold conditions came on with extreme slowness, the refrigerations being progressive from the Eocene period to the climax," seems to me to be a fatal one. At the same time, rather than asking with the above writers "the aid of astronomers and physicists in the solution of this problem," I would direct the attention of stratigraphical geologists to it, believing that, by steady accumulation of facts, they are more likely than anyone else to furnish the true clue to the solution of the glacial problem.

I have elsewhere called attention to marked changes in the faunas of the sedimentary rocks when passing from lower to higher levels, without the evidence of any apparent physical break, or any apparent change in the physical conditions, so far as can be judged from the lithological characters of the strata, and have suggested that such sudden faunistic variations may be due to climate. I refer to the matter as one which may well occupy the attention of local observers.

One of the most interesting points connected with climatic conditions is that of the former general lateral distribution of organisms, and its dependence upon the distribution of climatic zones. The well-known work of the late Dr. Neumayr[2] has, in the opinion of many geologists, established the existence of climatic zones whose boundaries ran practically parallel with the equator in Jurassic and Cretaceous times, and the possible existence of similar climatic zones in Palæozoic times has been elsewhere suggested; but it is very desirable that much more work should be done on this subject, and it can only be carried out by paying close attention to the vertical and lateral distribution of organisms in the stratified rocks.

So far we have chiefly considered the importance of stratigraphical geology in connection with the inorganic side of nature. We now come to the bearing of detailed stratigraphical work upon questions concerning the life of the globe, and here the evidence furnished by the geologist particularly appeals to the general educated public as well as to students of other sciences.

Attention has just been directed to the probable importance of former climatic changes in determining the distribution of organisms; but the whole subject of the geographical distribution of organisms during former geological periods, though it has already received a considerable amount of attention, will doubtless have much further light thrown upon it as the result of careful observations carried out amongst the stratified rocks.

So long ago as 1853, Pictet laid it down as a palæontological law that "the geographical distribution of species found in the strata was more extended than the range of species of existing faunas." One would naturally expect that at a time when the diversity of animal organization was not so great as it now is, the species, having fewer enemies with which to cope, and on the whole not too complex organizations to be affected by outward circumstances, would spread further laterally than they now do; but as we know that in earliest Cambrian times the diversity of organization was very considerable, it is doubtful whether any appreciable difference would be exerted upon lateral distribution then and now, owing to this cause. At the time at which Pictet wrote, the rich fauna of the deeper parts of the oceans, with its many widely distributed forms of life, was unknown, and the range in space of early organisms must have then struck everyone who thought upon the subject

[1] J. W. Gray and P. F. Kendall, "The Cause of an Ice Age": Brit. Assoc. Rep. (1892), p. 708.

[2] M. Neumayr, "Ueber klimatische Zonen während der Jura- und Kreidezeit": Denkschr. der math.-naturwissen. Classe der k. k. Akad. der Wissenschaften, vol. xlvii. Vienna, 1883.

as being greater than that of the shallow-water organisms of existing seas, which were alone known. It is by no means clear, however, with our present knowledge, that Pictet's supposed law holds good, and it will require a considerable amount of work before it can be shown to be even apparently true. Our lists of the fossils of different areas are not sufficiently complete to allow us to generalize with safety, but a comparison of the faunas of Australia and Britain indicates a larger percentage of forms common to the two areas, as we examine higher groups of the geological column. If this indication be fully borne out by further work, it will not prove the actual truth of the law, for the apparent wider distribution of ancient forms of life might be due to the greater probability of elevation of ancient deep-sea sediments than of more modern ones which have not been subjected to so many elevatory movements. Still, if the law be apparently true, it is a matter of some importance to geologists; and I have touched upon the subject here in order once again to emphasize the possibility of correlating comparatively small thicknesses of strata in distant regions by their included organisms.

Mention of Pictet's laws, one of which states that fossil animals were constructed upon the same plan as existing ones, leads me to remark upon the frequent assumption that certain fossils are closely related to living groups, when the resemblance between the hard parts of the living and extinct forms are only of the most general character. There is a natural tendency to compare a fossil with its nearest living ally, but the comparison has probably been often pushed too far, with the result that biologists have frequently been led to look for the ancestors of one living group exclusively amongst forms of life which are closely related to those of another living group. The result of detailed work is to bring out more and more prominently the very important differences between some ancient forms and any living creature, and to throw doubts on certain comparisons; thus I find several of the well-known fossils of the Old Red Sandstone, formerly referred without hesitation to the fishes, are now doubtfully placed in that class.

The importance of detailed observation in the field is becoming every day more apparent, and the specialist who remains in his museum examining the collections amassed by the labours of others, and never notes the mode of occurrence of fossils in the strata, will perhaps soon be extinct, himself an illustration of the principle of the survival of the fittest. In the first place, such a worker can never grasp the true significance of the changes wrought on fossil relics after they have become entombed in the strata, especially amongst those rocks which have been subjected to profound earth-movements; and it is to be feared that many "species" are still retained in our fossil lists whose supposed specific characters are due to distortion by pressure. But a point of greater importance is, that one who confines his attention to museums cannot, unless the information supplied to him be very full, distinguish the differences between fossils which are variations from a contemporaneous dominant form, such as "sports," and those which have been termed "mutations," which existed at a later period than the forms which they resemble. The value of the latter to those who are attempting to work out phylogenies is obvious, and their nature can only be determined as the result of very laborious and accurate field-work; but such labour in such a cause is well worth performing. The student of phylogeny has had sufficient warning of the dangers which beset his path, from an inspection of the various phylogenetic trees, constructed mainly after study of existing beings only, so

> ". . . like the borealis race,
> That flit ere you can point their place":

but recent researches amongst various groups of fossil organisms have further illustrated the danger of theorizing upon insufficient data, especially suggestive being the discovery of closely similar forms which were formerly considered to be much more nearly related than now proves to be the case. Thus Dr. Mojsisovics [1] has shown that Ammonites once referred to the same species are specifically distinct, though their hard parts have acquired similar structures, sometimes contemporaneously, sometimes at different times ; and Mr. S. S. Buckman [2] has observed the same thing, which he speaks of as "heterogenetic homœomorphy" in the case of certain brachiopods ; whilst Professor H. A. Nicholson and I [3] have given reasons for supposing that such heterogenetic homœomorphy, in the case of the graptolites, has sometimes caused the inclusion in one genus of forms which have arisen from two distinct genera. As the result of careful work, dangers of the nature here suggested will be avoided, and our chances of indicating lines of descent correctly will be much increased. It must be remembered that, however plausible the lines of descent indicated by students of recent forms may be, the actual links in the chains can only be discovered by examination of the rocks, and it is greatly to be desired that more of our geologists, who have had a thorough training in the field, should receive in addition one as thorough in the zoological laboratory. Shall I be forgiven if I venture on the opinion that a certain suspicion which some of my zoological fellow-countrymen have of geological methods, is due to their comparative ignorance of palæontology, and that it is as important for them to obtain some knowledge of the principles of geology as it is for the stratigraphical palæontologist to study the soft parts of creatures whose relatives he finds in the stratified rocks ?

The main lines along which the organisms of some of the larger groups have been developed, have already been indicated by several palæontologists, and detailed work has been carried out in several cases. As examples, let me allude to the trilobites, of which a satisfactory natural classification was outlined by the great Barrande in those volumes of his monumental work which deal with the fossils of this order, whilst further indication of their natural inter-relationships has been furnished by Messrs. C. D. Walcott, G. F. Matthew, and others ; to the graptolites, whose relationships have been largely worked out by Professor C. Lapworth, *facile princeps* amongst students of the Graptolitoidea, to whom we look for a full account of the phylogeny of the group ; to the brachiopods, which have been so ably treated by Dr. C. E. Beecher,[4] largely from a study of recent forms, but also after careful study of those preserved in the fossil state ; and to the echinoids and lamellibranchs, whose history is being extensively elucidated by Dr. R. T. Jackson,[5] by methods somewhat similar to those pursued by Dr. Beecher. I might give other instances,[6] but have chosen some striking ones, four of which especially illustrate the great advances which are being made in the study of the palæontology of the invertebrates by our American brethren.

[1] E. Mojsisovics, Abhandl. der k. k. geol. Reichsanst., vol. xvi (1893).

[2] S. S. Buckman, Quart. Journ. Geol. Soc., vol. li (1895), p. 456.

[3] H. A. Nicholson and J. E. Marr, Geol. Mag., Dec. 4, Vol. II (1895), p. 531.

[4] C. E. Beecher, "Development of the Brachiopoda" : Amer. Journ. Sci., ser. iii, vol. xli (1891), p. 343, and vol. xliv (1892), p. 133.

[5] R. T. Jackson, "Phylogeny of the Pelecypoda," Mem. Boston Soc. Nat. Hist., vol. iv (1890), p. 277 ; and "Studies of Palæechinoidea," Bull. Geol. Soc. Amer., vol. vii (1896), p. 171.

[6] *E.g.* the following papers treating of the Cephalopoda : A. Hyatt, "Genesis of the Arietidæ," Smithsonian Contributions, vol. xxvi (1889) ; M. Neumayr, Jura-Studien I, "Ueber Phylloceraten," Jahrb. der k. k. Geol. Reichsanst., vol. xxi (1871), p. 297 ; L. Würtenberger, "Studien über die Stammesgeschichte der Ammoniten," Leipzig, 1880 ; S. S. Buckman, "A Monograph of the Inferior Oolite Ammonites of the British Islands," 1887, Monogr. Palæontographical Soc.

I have occupied the main part of my address with reasons for the need of conducting stratigraphical work with minute accuracy. Many of you may suppose that the necessity for working in this way is so obvious that it is a work of supererogation to insist upon it at great length; but experience has taught me that many geologists consider that close attention to details is apt to deter workers from arriving at important generalizations, in the present state of our science. A review of the past history of the science shows that William Smith, and those who followed after him, obtained their most important results by steady application to details, and subsequent generalization, whilst the work of those who theorize on insufficient data is apt to be of little avail, though often demanding attention on account of its very daring, and because of the power of some writers to place erroneous views in an attractive light, just as

> " . . . . the sun can fling
> Colours as bright on exhalations bred
> By weedy pool or pestilential swamp,
> As on the rivulet, sparkling where it runs,
> Or the pellucid lake."

Nor is there any reason to suppose that it will be otherwise in the future, and I am not one of those who consider that the brilliant discoveries were the exclusive reward of the pioneers in our science, and that labourers of the present day must be contented with the gleanings of their harvest; on the contrary, the discoveries which await the geologist will probably be as striking as are those which he has made in the past. The onward march of science is a rhythmic movement, with now a period of steady labour, anon a more rapid advance in our knowledge. It would perhaps be going too far to say that, so far as our science is concerned, we are living in a period rather of the former than of the latter character, though no great geological discovery has recently affected human thought in the way in which it was affected by the proofs of the antiquity of man, and by the publication of "The Origin of Species." If, however, we are to some extent gathering materials, rather than drawing far-reaching conclusions from them, I believe this is largely due to the great expansion which our science has undergone in recent years. It has been said that geology is "not so much one science, as the application of all the physical sciences to the examination and description of the structure of the earth, the investigation of the agencies concerned in the production of that structure, and the history of their action"; and the application of other sciences to the elucidation of the history of our globe has been so greatly extended of recent years, that we are apt to lose sight of the fact that geology is in itself a science, and that it is the special province of the geologist to get his facts at first hand from examination of the earth. The spectroscope and the telescope tell the geologist much; but his proper instrument is the hammer, and the motto of every geologist should be that which has been adopted for the Geological Congress—"*Mente et malleo.*"

At the risk of being compared to a child playing with edge tools, I cannot help referring to the bearing of modern stratigraphical research on the suggested replacement of a school of uniformitarianism by one of evolution. The distinguished advocate of Evolutionism, who addressed the Geological Society in 1869 upon the modern schools of geological thought, spoke of the school of evolution as though it were midway between those of uniformitarianism and catastrophism, as indeed it is logically, though, considering the tenets of the upholders of catastrophism, as opposed to those of uniformitarianism, at the time of that address, there is no doubt that evolutionism was rather a modification of the uniformitarianism of the period than intermediate between it and catastrophism, which was then practically extinct, at any rate in Britain. One

of my predecessors in this chair, speaking upon this subject, says that "the good old British ship 'Uniformity,' built by Hutton and refitted by Lyell, has won so many glorious victories in the past, and appears still to be in such excellent fighting trim, that I see no reason why she should haul down her colours, either to 'catastrophe' or 'evolution.'" It may be so; but I doubt the expediency of nailing those colours to the mast. That Lyell, in his great work, proved that the agents now in operation, working with the same activity as that which they exhibit at the present day, *might* produce the phenomena exhibited by the stratified rocks, seems to be generally admitted, but that is not the same thing as proving that they *did* so produce them. Such proof can only be acquired by that detailed examination of the strata which I have advocated in this address, and at the time that the last edition of the "Principles" appeared, our knowledge of the strata was far less complete than it has subsequently become. It appears to me that we should keep our eyes open to the possibility of many phenomena presented by rocks, even newer than the Archæan rocks, having been produced under different conditions from those now prevalent. The depths and salinity of the oceans, the heights and extent of continents, the conditions of volcanic action, and many other things, may have been markedly different from what they are at present, and it is surely unphilosophical to assume conditions to have been generally similar to those of the present day, on the slender data at our disposal. Lastly, uniformitarianism, in its strictest sense, is opposed to rhythmic recurrence of events. "Rhythm is the rule with nature; she abhors uniformity more than she does a vacuum," wrote Professor Tyndall, many years ago, and the remark is worth noting by geologists. Why have we no undoubted signs of Glacial epochs amongst the strata from early Cambrian times to the Great Ice-period, except in Permo-Carboniferous times? Is there not an apparent, if not a real, absence of manifestation of volcanic activity over wide areas of the earth in Mesozoic times? Were not Devonian, Permo-Triassic, and Miocene times periods of mountain-building over exceptionally wide areas, whilst the intervening periods were rather marked by quiet depression and sedimentation? A study of the evidence available in connection with questions like these suggests rhythmic recurrence. Without any desire to advocate hasty departure from our present methods of research, I think it should be clearly recognized that evolution may have been an important factor in changing the conditions even of those times of which the geologist has more direct knowledge. In this, as in many other questions, it is best to preserve an open mind; indeed, I think that geologists will do well to rest satisfied without an explanation to many problems, amongst them the one just referred to; and that working hypotheses, though useful, are better retained in the manuscript notebooks of the workers than published in the Transactions of learned societies, whence they filter out into popular works, to the great delight of a sceptical public should they happen to be overthrown.

May I trespass upon your patience for one moment longer? As a teacher of geology, with many years' experience in and out of a large University, I have come to the conclusion that geology is becoming more generally recognized as a valuable instrument of education. The memory, the reasoning faculties, and the powers of observation are alike quickened. The work in the open air, which is inseparable from a right understanding of the science, keeps the body in healthy condition. But over and above these benefits, the communing with nature, often in her most impressive moods, and the insignificance of events in a man's lifetime, as compared with the ceaseless changes through the long æons which have gone before, so influence man's moral nature, that they drive out his meaner thoughts and make him "live in charity with all men."